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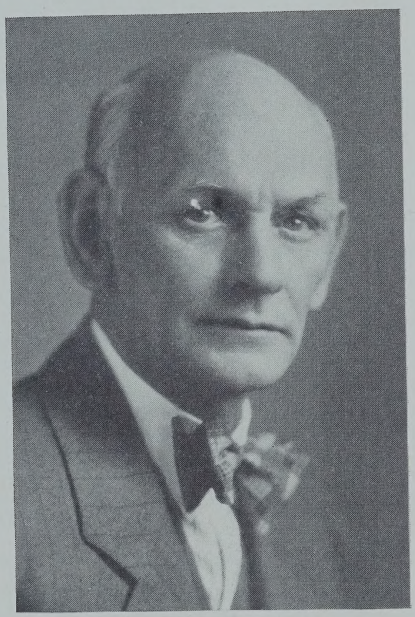

SHEPHERD IVORY FRANZ

MAY 27, 1874—OCTOBER 14, 1933

By the death of Shepherd Ivory Franz, American Psychology has lost one of its outstanding figures. Those who are curious as to the details of his life can readily find them in *Who's Who, American Men of Science,* and the *Psychological Register.* Here, I wish only to present an appreciation of him. Professionally, Franz was one of the group which made modern psychology what it is. Devoted to no *-ism,* he was at once an iconoclast and a builder. His greatest work, undoubtedly, was the freeing of psychology from the primitive superstition of fixed "brain centers." His experimental work, in showing that functions usually dependent upon definite cortical areas may be restored after the areas are destroyed, was startling. We now accept the fact as a commonplace. Curiously enough, little has been added to our knowledge concerning brain areas since his work. The problem still awaits, for further solution, a psychologist with a mind as keen as his.

Personally, his death is a severe loss to all those who were fortunate enough to know him. Tart, but generous and kindly; incisive and free-speaking, but humanly sympathetic; earnest and thorough in his work, but a jolly companion; he gave an example of balance and proportion to which few can approximate. Those of us who knew him as a man extend our spontaneous sympathy and affection to his bereaved wife and daughter, while realizing our own irreparable loss.

KNIGHT DUNLAP

THE DURATION AND INTENSITY OF THE EXPOSURE LIGHT AS FACTORS IN DETERMINING THE COURSE OF THE SUBSEQUENT DARK-ADAPTATION:

I. THE MATCHING METHOD*

From the Psychology Laboratory of Skidmore College

DOROTHEA E. JOHANNSEN

I. INTRODUCTION

The problem of the relationship between the duration of exposure to, and the intensity of, visual stimuli as determiners of sensitivity has received considerable attention from psychologists, but the emphasis has been laid upon very short durations. When the time is lengthened to a period varying from several seconds to several minutes, we approach certain aspects of the problem of what is called "visual fatigue" or "light-adaptation." Similarly, a problem is found when we consider the manner in which sensitivity increases when the eye remains unstimulated, i.e., dark-adaptation. Although there is a considerable number of investigations of light- and dark-adaptation, very few of them have attempted to determine the relationship existing between the two phenomena; essentially, they are two aspects of the same physiological process. Hecht's (5) work on dark-adaptation does take account of the eye's state by exposing the eye for a definite period of time to a screen illuminated to a given brightness. His results, for the rod-free area, show that dark-adaptation proceeds very precipitately during the first few moments, and then gradually slows down; indeed, most of the process occurs during the first 30 seconds, and it practically ceases after 10 minutes. Blanchard (1) used pre-adaptation fields of four different intensities; when the brightness of the pre-adaptation field is low, dark-adaptation occurs rapidly at first, then more slowly. When the brightness of the pre-adaptation field is high, the curve of dark-adaptation approaches a straight line.

*Accepted for publication by Carl Murchison of the Editorial Board.

The results of the studies of light-adaptation have indicated that the decrease in sensitivity following stimulation follows a comparable course, i.e., sensitivity decreases rapidly at first, then more slowly, and a stable equilibrium is gradually attained. Von Kries (7) seems to have been the earliest investigator who was interested in and reported quantitative experimental evidence on both light- and dark-adaptation. His results showed that light-adaptation goes on rapidly at first, then more slowly, and the greater the intensity of the light, the more rapidly such adaptation occurs. His results for the course of dark-adaptation are not so consistent or extensive, but he found the form of the curve to be similar to that for the reverse process, i.e., rapid at first with a gradual slowing-down. The *longer* the previous exposure, the *more abruptly* the eye recovers, complete dark-adaptation occurring in almost the same length of time for all three of the exposure times used. Lohmann (8) showed that the change in sensitivity which occurs during light-adaptation is most rapid during the first two minutes, regardless of the exposure time or the intensity of the stimulus light, although the absolute sensitivity of the eye depends upon both factors. Geldard's (3) results on light-adaptation cover a wider range than either von Kries or Lohmann used, but they confirm the results obtained by the earlier experimenters. Although Geldard did not answer the question regarding the relative rates of dark-adaptation following different exposure intensities, the present author (6) attempted to do so, using Geldard's apparatus and the same intensities. The evidence indicated that adaptation takes place rapidly at first, then slows down, and the higher the intensity, the longer the time required for complete dark-adaptation. Møller (9, 10) studied both light- and dark-adaptation, by means of the Tscherning glasses, and, although his curves are plotted in semi-logarithmic form, they also show that adaptation of both kinds takes place rapidly at first, and then slows down. He found that the eye requires as long a time to light-adapt as it originally required to completely dark-adapt, and he emphasizes the reciprocal relationship between light- and dark-adaptation.

The evidence may be summarized as follows: The change in the sensitivity of the eye in adapting both to light and to dark is rapid during the first few seconds, after which it gradually slows down, until no further change occurs. In the case of light-adapta-

tion, the evidence is clear that the greater the intensity of the light, the more rapid and the more marked the reduction in sensitivity. The relation of the course of dark-adaptation to the intensity of the preceding stimulation has not been so accurately determined, but the indication is that the eye dark-adapts more rapidly after exposure to a dim light than to a bright one. The effect of the duration of the exposure light on subsequent dark-adaptation has not been determined; Møller found that it requires the same length of time to light-adapt as it originally required to dark-adapt.

II. Problem

The problem of the present study was to determine the precise way in which the intensity and duration of the light to which the eye has just been light-adapted are factors in determining the course of the subsequent dark-adaptation.

Changes in level of sensitivity may be either toward greater sensitivity or toward less sensitivity. In this discussion we shall consistently use the term "light-adaptation" to indicate the change toward less sensitivity; this has sometimes been called "visual fatigue," "negative adaptation," or "desadaptation." The term "dark-adaptation" will be used to refer to the change toward increased sensitivity, which is sometimes called "recovery from visual fatigue," "positive adaptation," or "minuthesis."

Because of the evidence that dark-adaptation is of the same general type in both the fovea and the periphery of the eye, but occurs much more rapidly at the fovea (Hecht, 8), we used a stimulating area which would be included within the macula; it subtended a visual angle of 3° 10′.

III. Apparatus

Because of the evidence extant indicative of a mutual dependence of the parts of the same retina (Geldard, 3; Cobb, 2), the apparatus was so arranged that the light-adaptation induced was measured by comparing the brightness of the light-adapting stimulus on the left eye with a stimulus of identical size and shape on the right (dark-adapted) eye.

The observations were made in a dark-room. A stereoscopic hood, surrounded by a heavy black cloth, shielded O's eyes from any stray light. Artificial pupils with a radius of 1 mm., were set

in the openings of the stereoscope, from which the lenses had been removed.

Two tubes, 32.2 cm. in length, blackened both inside and out with lamp black, extended from the hood to a point 3.5 cm. from the center of the illuminated blocks. The openings at the ends of the tubes, which permitted a view of the magnesium carbonate stimulus blocks, were semicircles, 2 cm. in diameter, with the straight edge on the nasal side in each case.

The stimulus blocks were of magnesium carbonate, with smooth surfaces, 4.5 by 5.2 cm. in size. They were so set that the line of sight and the light from the source both struck the center at an angle of 45°.

The sources of light were two Mazda electric lamps, standardized by the Research Department of the General Electric Company. The standard light, a 200-watt flood lamp, was calibrated at 302 mean horizontal candle-power through a 5-cm. square opening. The comparison light was 281 mean horizontal candle-power, through a 5-cm. square opening. Throughout the experiment the standard light remained at a constant distance of 25 cm. from the center of the magnesium carbonate surface. Assuming the reflection coefficient of the magnesium carbonate to be 98% (Taylor, 20), we find the stimulating intensity of the light to be 1065.67 candles per square meter. This intensity is referred to as 1/1 in the following discussion. Two lower intensities of light were used. The brightness in each case was reduced by means of a neutral filter, 5 cm. on a side, one with a transmission value of 1/1000, the other with a transmission value of 1/10; both were standardized to within an error of 1% by the Research Department of the Eastman Kodak Company. These intensities will be referred to as 1/1000 and 1/10, respectively.

The lights were carried in wooden boxes, 20.6 x 11.2 x 14 cm. The left one (standard) was stationary, but the right one (comparison) was constructed of light wood and rode on runners along a meter stick, which permitted adjustment of the intensity. The front of each box was closed with a tin cover, with an opening (5 x 5 cm.) into which the neutral filters could be inserted when one of the lower intensities was being used.

A heavy black cardboard screen separated the two magnesium carbonate blocks, and prevented light from one stimulus affecting the eye on the opposite side.

IV. Procedure

After a preliminary period of dark-adaptation of at least 6 minutes, which was taken as being adequate since only the rod-free area of the eye was being used (Hecht, 6), O was given a "ready-now" signal and the adapting light (on the left) turned on for a specified length of time. (In some series this was 15 sec., in the others, 30 sec.) At the end of this time, after a warning, the light was turned off. O was required to close his eyes at the signal indicating the end of the observation period, in order to prevent observing the gradual fading-out which it was impossible to eliminate with this big bulb. After a given interval for dark-adaptation, O was again signaled, and the lights were turned on simultaneously. O observed the two lights, and reported the right as being brighter, less bright, or equal to the left. Two seconds only were allowed for comparison. Previous investigations have indicated that judgments made in a short time are as reliable as those made when a longer time is permitted, and the shorter time reduces the light-adaptation which immediately sets in for both eyes (Reeves, 11; Geldard, 3). A method of minimal change was used; at least four determinations of the point of equality were made for each adaptation time. After each observation, from 3 to 5 minutes, depending upon the intensity and exposure time being used, were allowed for dark-adaptation.

Five individuals observed; they were untrained in this sort of observing and were given several training periods before the results were used.[1]

The apparent brightness was determined after the following time intervals: 0″, 5″, 10″, 15″, 30″, 60″, 120″, 180″, 240″, and 300″. In some cases, of course, it was not necessary to use all these intervals, adaptation being completed in less time. Between every two successive trials the eye was allowed to dark-adapt.

The highest intensity (1/1) gave a brightness on the stimulating surface of approximately 1066 candles per square meter. With this standard the comparison light was matched, when the eyes were completely dark-adapted. The comparison light was adjusted

[1]The author takes pleasure in acknowledging her debt to the individuals who gave many hours to acting as observers. They were: Misses E. Drey (D), C. Mansar (M), E. Niemann (N), and M. Pierpont (P), members of the Faculty of Skidmore College, and Miss I. Hicks (H), a student majoring in the Department of Psychology.

in brightness by moving it along the meter scale, and the place at which the light was set on the scale was considered the subjective equivalent of 1066 candles per square meter, and was used as a standard in computing the amount of reduction in brightness which took place with light-adaptation. A similar procedure was followed with the other intensities, except that the comparison light was also reduced by means of a filter of 1/100 transmission value before the scale adjustments were made.

V. Results

Reference to Tables 1 and 2 and to Figures 1 and 2 will indicate the comparative rates of dark-adaptation under the different

TABLE 1

Showing the Comparative Rates of Dark-Adaptation after Exposure to a Light of 1066 Candles per Square Meter, as Measured by the Apparent Brightness of the Light in Percentages of the Original Intensity

With 15″ light-adaptation (average results of H, M, and N)		With 30″ light-adaptation (average results of D and M)	
Time of adaptation	Apparent brightness	Time of adaptation	Apparent brightness
0″	55.0%	0″	18.7%
5″	61.4	5″	38.4
10″	68.7	10″	39.7
15″	74.2	15″	41.3
30″	84.6	30″	51.1
60″	90.0	60″	69.7
120″	95.5	120″	82.3
180″	99.4	180″	82.7
		240″	89.8

TABLE 2

Showing the Comparative Rates of Dark-Adaptation after Exposure to a Light of 1.066 Candles per Square Meter, as Measured by the Apparent Brightness of the Light in Percentages of the Original Intensity

With 15″ light-adaptation (average results of H, M, N, and P)		With 30″ light-adaptation (average results of D and M)	
Time of adaptation	Apparent brightness	Time of adaptation	Apparent brightness
0″	70.6%	0″	53.4%
5″	84.6	5″	70.8
10″	90.5	10″	84.1
15″	95.4	15″	90.5
30″	98.9	30″	100.1
60″	99.7		

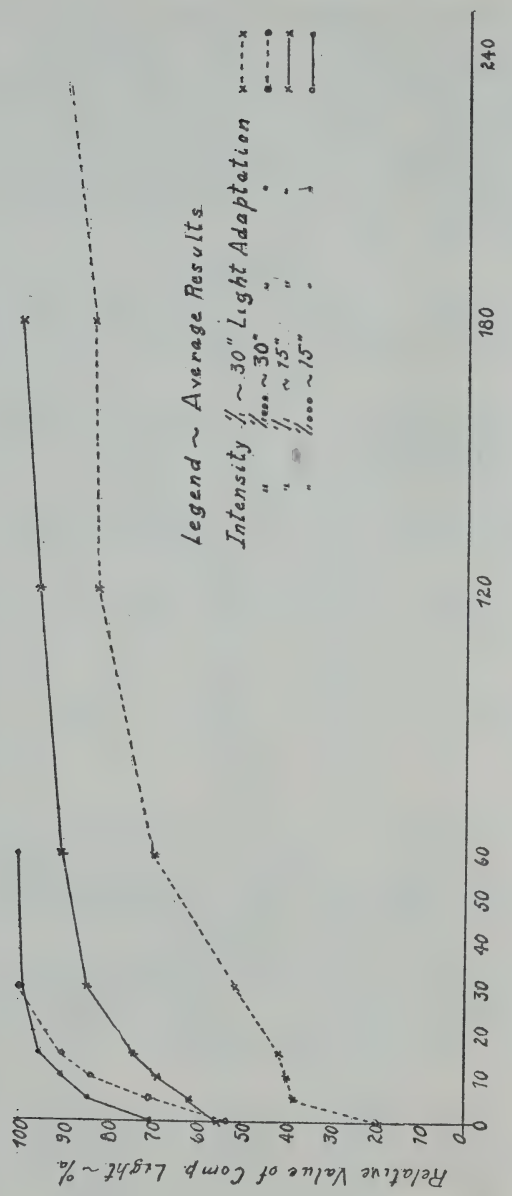
Legend ~ Average Results
Intensity ½ ~ 30" Light Adaptation
" ¼ ~ 30" "
" ½ ~ 15" "
" ¼ ~ 15" "
Relative Value of Comp Light ~ %
100
90
80
70
60
50
40
30
20
10
0
0 10 20 30 40 50 60 120 180 240
Seconds of Dark Adaptation

FIGURE 1

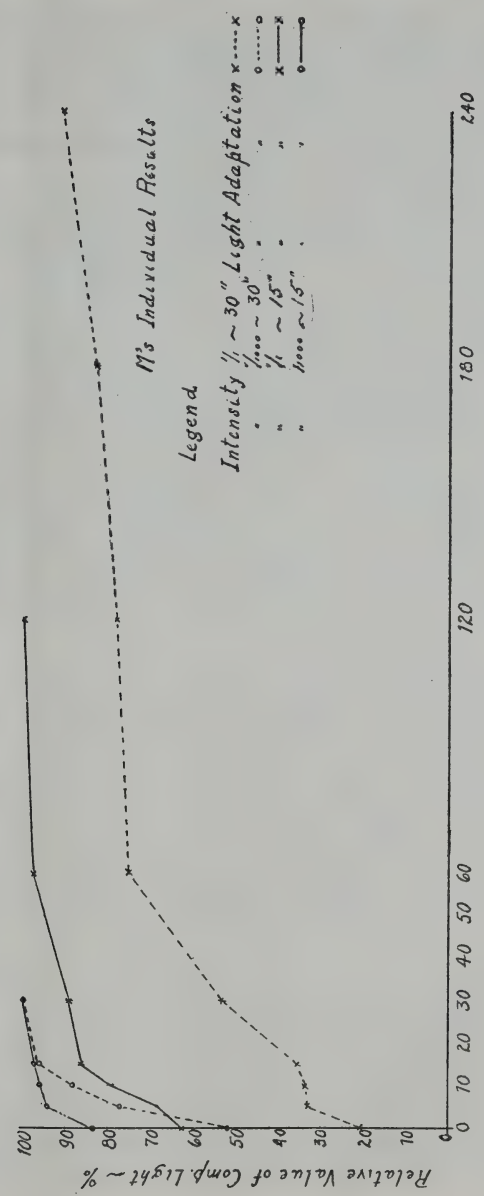
M's Individual Results
Legend
Intensity 1/1000 ~ 30" Light Adaptation x-----x
" 1/1000 ~ 30" " " o-----o
" 1/1 ~ 15" " " x——x
" 1/1000 ~ 15" " " o——o
Relative Value of Comp. Light ~ %
100
90
80
70
60
50
40
30
20
10
0
0 10 20 30 40 50 60
120
180
240
Seconds of Dark Adaptation

FIGURE 2

conditions used. The figures show the apparent brightness of the light after given periods of dark-adaptation, as *percentages of the original brightness.* An analysis of the figures shows us that the results agree with those of other investigators, in that the course of the dark-adaptation for all the conditions used is similar to the course of light-adaptation, i.e., the slope of the curve is at first great, and then it gradually approaches the horizontal. It is clear that both the intensity and the duration of the exposure light have an influence on the course of the subsequent dark-adaptation.

From the results we find:

1. On the basis of the same three O's (H, M, and N) in the series in which the duration of exposure was 15 sec., and of the same two O's (M and D) in the series in which the duration of exposure was 30 sec., we can say that the greater the intensity of the light to which the eye has been exposed, the greater the decrease in apparent brightness for the same duration of exposure, and the longer the eye has to remain in darkness in order to regain its former sensitivity. It might be pointed out that the brighter light (1/1) is 1000 times as intense as the fainter (1/1000), but for 30-sec. exposure time, the subsequent dark-adaptation process requires only 10 times as long for the greater as for the lesser intensity.

2. We can say that the longer the exposure time for the same intensity, the greater the decrease in apparent brightness and, in the case of the higher intensity, the longer the time required for complete dark-adaptation; for the lower intensity, on the other hand, the dark-adaptation is complete in the same length of time for both exposure durations. [This conclusion is based upon the results of only one O (M) who observed in all four series; when average results (only M having observed in all series) are compared, the shorter exposure time requires the longer adaptation. (Figures 1 and 2.)]

3. A comparison of the average curves showing dark-adaptation following exposure to a bright light with those showing adaptation after exposure to a dim light, when the duration of exposure has been the same, shows that the initial rate of increase in sensitivity is more rapid after the dim than after the bright light. This statement is true for 5 out of 6 individual comparisons as well as for the averages. When the brightness of the exposure light is the

same, but the duration of the exposure is different, the sensitivity increases relatively more rapidly during the first 30 sec. of adaptation for the longer durations, the difference, however, being more marked for the dimmer light. When M's individual curves are compared (since she was the only O who observed in all four series), this fact is brought out even more clearly than in the average curves. In other words, when the intensity of the stimulating light is constant, but the duration of exposure is different, the more rapid initial increase in sensitivity occurs after the longer time (i.e., after *greater* stimulation). When the duration of the stimulating light is constant, but the intensity is different, the more rapid initial increase in sensitivity occurs after exposure to the less bright light (i.e., after *less* stimulation). We may conclude, therefore, that in the present situation time and intensity are not interchangeable factors.

4. After 15 sec.'s exposure to the brighter light, the eye's sensitivity is reduced to approximately the same state, as measured in terms of apparent brightness, as after 30 sec.'s exposure to the dimmer light. It would seem reasonable, therefore, to suppose that the subsequent measurements of sensitivity after the different periods of dark-adaptation should be approximately the same. This, however, is not the case, for the two curves are quite different in form. This is true both for the average results and for M's individual results. (See Figures 1 and 2.) This result is a further justification of our conclusion above, i.e., that time and intensity are not interchangeable factors in producing a condition of the eye such that the course of the subsequent dark-adaptation in the two cases will be the same.

In the part of the experiment described above, the sensitivity of the eye was measured by determining the apparent brightness of the light after given periods of dark-adaptation. It is clear, however, that a change in sensitivity which will produce a certain reduction in the apparent brightness of a bright light will not necessarily produce the same reduction in the apparent brightness of a dim light; our figures, therefore, for the two intensities are not directly comparable. A part of the experiment was therefore repeated with the following modification: When the eye had been exposed to the brighter light, the decrease in apparent brightness was measured in terms of the lowest intensity by pushing the 1/1000 filter into

the holder in front of the exposure light. We also used another intensity (1/10); in both cases the eye was light-adapted for 15 sec. After each period of dark-adaptation the match was made with the 1/1000 filter in place; it was therefore possible directly to compare the apparent brightness of all three intensities in terms of one standard. (See Tables 2, 3, and 4, and Figure 3.)

The results obtained again show that the dark-adaptation process goes on rapidly at first, and then more slowly, and it is completed

TABLE 3

SHOWING THE COMPARATIVE RATES OF DARK-ADAPTATION AFTER EXPOSURE TO A LIGHT OF 1066 CANDLES PER SQUARE METER, AS MEASURED BY THE APPARENT BRIGHTNESS OF THE LIGHT IN PERCENTAGES OF THE BRIGHTNESS OF A LIGHT OF 1.066 CANDLES PER SQUARE METER

With 15″ light-adaptation (Observer P)	
Time of adaptation	Apparent brightness
0″	38.8%
5″	47.7
10″	54.9
15″	56.9
30″	65.8
60″	76.9
120″	81.8
180″	88.4
240″	100.0

TABLE 4

SHOWING THE COMPARATIVE RATES OF DARK-ADAPTATION AFTER EXPOSURE TO A LIGHT OF 106.6 CANDLES PER SQUARE METER, AS MEASURED BY THE APPARENT BRIGHTNESS OF THE LIGHT IN PERCENTAGES OF THE BRIGHTNESS OF A LIGHT OF 1.066 CANDLES PER SQUARE METER

With 15″ light-adaptation (Average results of H and P)	
Time of adaptation	Apparent brightness
0″	47.8%
5″	55.9
10″	62.3
15″	65.2
30″	70.3
60″	77.0
120″	87.0
180″	91.9
240″	99.8

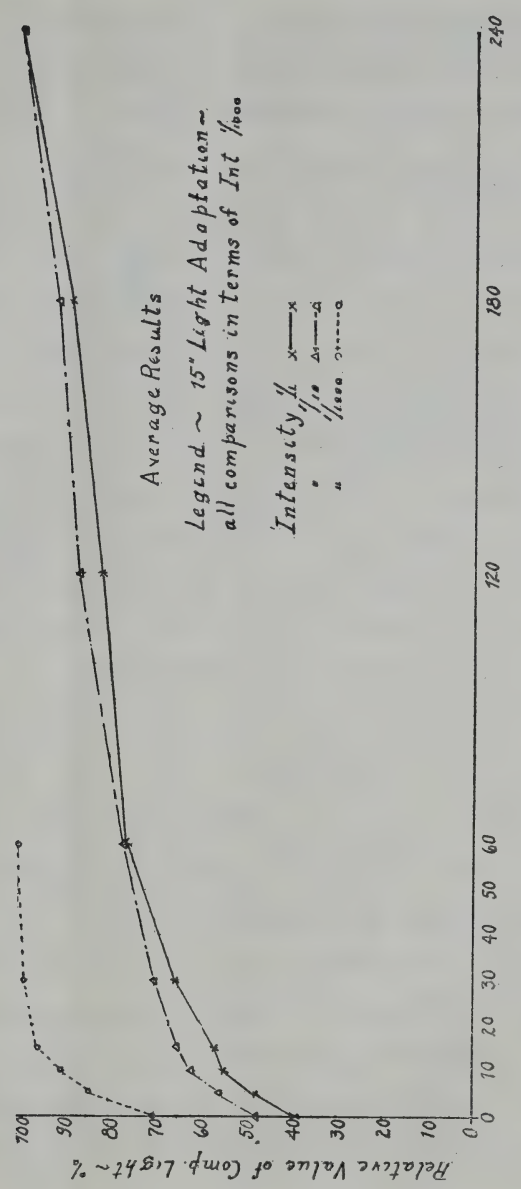
Average Results
Legend ~ 15" Light Adaptation ~
all comparisons in terms of Int 1/1000
Intensity 1/
1/10
1/1000
Relative Value of Comp. Light ~ %
Seconds of Dark Adaptation
0
10
20
30
40
50
60
120
180
240
70
80
90
50
40
30
20
10
0

FIGURE 3

more quickly for the less bright light. There is found to be comparatively little difference between the two higher intensities, 1/1 and 1/10; this would indicate that the intensities are too close together in value to produce a significantly different curve of adaptation.

It is to be regretted that the curve of adaptation after 15 sec.'s exposure to the highest intensity (1/1) when the comparison was made in terms of the apparent brightness of the dimmest light (1/1000) could be obtained on only one O. Since this O had not observed in the series in which the comparison was made in terms of the original light, the figures are not directly comparable, but it is clear that the shapes of the two curves are almost identical.

A comparison of the three curves makes it clear that the adaptation which follows exposure to a bright light goes on more slowly than the adaptation which fol'ows exposure to a dim one, and the total time required for complete adaptation is longer. Considering the relative brightness involved, we may say that the time for dark-adaptation is not a linear function of the brightness, for with intensity 1/1 adaptation is complete in 4 minutes, with intensity 1/1000 it is complete in 1 minute, which is one-fourth, instead of one-thousandth of the time required by the higher intensity.

VI. Summary and Conclusion

Under the conditions of our experiment, we find that the decrease in sensitivity (as measured by loss of brightness) is greater for the higher intensities and the longer exposure times. This agrees with the results obtained by earlier investigators. But when we analyze the curves which indicate the course of the increase in sensitivity when the eye remains unstimulated we observe the following facts:

1. The greater the intensity of the exposure light, the longer does the subsequent dark-adaptation process take.
2. Under the same conditions, the initial rate of increase in sensitivity is greater for the dimmer light.
3. The longer the duration of exposure for the higher intensity, the longer the subsequent dark-adaptation process; for the lower intensity the subsequent dark-adaptation process requires approximately the same length of time for both durations of exposure.
4. Under the same conditions, the initial rate of increase in sensitivity is greater for the longer durations.

5. The shape of the curve showing dark-adaptation depends upon whether time or intensity was the chief factor in determining the initial sensitivity of the eye.

We therefore conclude that the two factors of time and intensity are not interchangeable in producing the characteristic of the eye which determines the course which the subsequent dark-adaptation will take.

The results upon which this conclusion is based was obtained by measuring the decrease in sensitivity by matching the apparent brightness of the light on the exposed eye with the brightness of the light on the other (dark-adapted) eye. This method has been used in such experiments, and was chosen here because of its precision, and because it was mechanically easier to handle than the more usual threshold method. Sensitivity is commonly measured by the determination of the least amount of stimulus which is required to arouse a response (i.e., the stimulus limen). Fundamentally, there seems no reason for assuming that either one of these methods should be any more valid than the other as a measure of sensitivity. We find such a striking difference from the effects of duration and intensity of exposure, however, that it seemed worth while to repeat the experiment with the threshold technique, in order to see whether our conclusions would be substantiated by that method. The results of that experiment will be reported in a subsequent paper.

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LA DURÉE ET L'INTENSITÉ DE LA LUMIÈRE EXPOSÉE COMME FACTEURS DANS LA DÉTERMINATION DU COURS DE L'ADAPTATION SUBSÉQUENTE À L'OBSCURITÉ: I. LA MÉTHODE DE COMPARAISON

(Résumé)

Le problème de cette étude a été de déterminer le moyen exact dont l'intensité et la durée de la lumière à laquelle l'oeil vient d'être adapté sont des facteurs dans la détermination du cours de l'adaptation subséquente à l'obscurité. L'oeil gauche a été adapté à la lumière du champ semi-circulaire d'exposition pour une durée définie. Ensuite on a déterminé la clarté apparente après des durées données de l'adaptation à l'obscurité, au moyen de comparer la clarté de ce champ avec la clarté d'un champ de grandeur et de forme identiques qui a stimulé l'oeil droit (adapté à l'obscurité). Les yeux ont été adaptés à l'obscurité entre chaque paire d'épreuves successives. Les résultats montrent que plus l'intensité de la lumière d'exposition est grande, plus l'adaptation à l'obscurité a besoin de temps; la première vitesse de l'accroissement de la sensitivité est plus grande pour la lumière plus faible. Plus la durée de l'exposition à la plus grande intensité est longue, plus l'adaptation subséquente à l'obscurité a besoin de temps; pour la plus petite intensité approximativement la même durée est nécessaire pour les deux durées. La vitesse initiale de l'accroissement de la sensitivité est plus grande pour les plus longues durées. Puisque la courbe qui montre l'adaptation à l'obscurité dépend de ceci: le temps ou l'intensité a-t-il été le facteur principale dans la détermination de la sensitivité initiale de l'oeil, on peut conclure que ces deux facteurs ne peuvent pas être changés l'un pour l'autre.

Johannsen

DIE DAUER UND INTENSITÄT DES EXPOSITIONLICHTES ALS FAKTOREN IN DER BESTIMMUNG DES VERLAUFS DER NACHFOLGENDEN DUNKELADAPTION: I. DIE PAARUNGSMETHODE

(Referat)

Das Problem der gegenwärtigen Untersuchung bestand darin, die genaue Art und Weise zu bestimmen, unter welcher die Intensität und Dauer des Lichts, worauf das Auge eben helladaptiert wurde, Faktoren sind, in der Bestimmung des Verlaufs der nachfolgenden Dunkeladaption. Das linke Auge war durch ein halbkreisförmiges Expositionsfeld während einer bestimmten Periode lichtadaptiert. Die scheinbare Helligkeit wurde dann nach gegebenen Perioden der Dunkeladaption dadurch bestimmt, dass man die Helligkeit dieses Feldes mit der Helligkeit eines gleichgrossen und gleichgeformten Feldes verglich, das das rechte (dunkeladaptierte) Auge reizte. Die Augen waren zwischen je zwei aufeinanderfolgenden Versuchen dunkeladaptiert. Die Ergebnisse zeigen, dass der Prozess der Dunkeladaption um so länger dauert, je grösser die Intensität des Expositionlichtes ist; die anfängliche Zunahme in der Empfindlichkeit ist grösser für das mattere Licht. Je länger die Expositionsdauer auf die grössere Intensität ist, um so länger dauert der nachfolgende Prozess der Dunkeladaption; für eine niedrigere Intensität wird für beide Zeitabschnitte ungefähr dieselbe Zeit gebraucht. Die individuelle Proportion der Zunahme der Empfindlichkeit ist grösser für die längern Zeitabschnitte. Da die Kurve der Dunkeladaption davon abhängt, ob Zeit oder Intensität der Hauptfaktor in der Bestimmung der anfänglichen Empfindlichkeit des Auges war, dürfen wir schliessen, dass die zwei Faktoren nicht vertauschbar sind.

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THE DURATION AND INTENSITY OF THE EXPOSURE LIGHT AS FACTORS IN DETERMINING THE COURSE OF THE SUBSEQUENT DARK-ADAPTATION:

II. THRESHOLD METHOD*

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I. INTRODUCTION

The present experiment was undertaken in order to determine whether the threshold method of measuring the sensitivity of the eye will produce radically different results from those obtained by measuring sensitivity by matching apparent brightnesses. In an earlier study (3) an effort was made to determine the precise relation which holds between the duration and intensity of the exposure light and the course of the subsequent dark-adaptation. The following method was used: The apparatus was so arranged that the light stimulated the left eye, in the rod-free area only. After a period of exposure (15 sec. in some series, 30 sec. in others), the light was turned off, and, after varying periods of dark-adaptation, the apparent brightness of the light determined by matching it with a light which stimulated a comparable area of the right (completely dark-adapted) eye. Three different intensities were used (1066, 106.6, and 1.066 candles per square meter, or 342, 34.2, and .0342 millilamberts). Two chief results were obtained:

1. The brighter the light (duration of exposure the same) the more slowly does the subsequent dark-adaptation start, and the longer does it take to complete it.

2. The longer the exposure period (intensity the same) the more rapid is the initial rate of the subsequent dark-adaptation; in the

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case of the higher intensity a longer exposure time requires a longer recovery period, but for the lower intensity the reverse is the case.

In view of the reciprocal relationship which has been found to hold between time and intensity, when the durations are very short, we might expect a comparable, if not identical, situation here, i.e., that, to some extent at any rate, a dim light of long exposure would produce results similar to the results produced by a relatively short exposure to a bright light. An analysis of the curves obtained in the previous experiment indicated very definitely that the curve showing the course of dark-adaptation which followed the long exposure to a dim light was quite different from the curve following short exposure to a bright light, even though the two curves start from approximately the same point in terms of apparent brightness. This brought up the question of whether the results obtained were a function of the method used or whether the more commonly used threshold technique would produce similar ones. It was consequently decided to repeat the experiment with the threshold technique in order to see whether the results would be duplicated with the different method.

II. Apparatus[1]

The apparatus used in the present experiment consisted of two parts, an exposure area and a system for determining the threshold. The whole apparatus was in a dark-room; E worked behind a heavy curtain and used only a small flash-light in making records, adjustments, etc. The exposure area subtended an angle of 1.46° at the eye, and therefore fell well within the rod-free area; it was of opal glass, illuminated from behind by a 250-watt light, run on 120 volts D. C.; its brightness was determined photometrically to be 98 candles per square meter (31 millilamberts). The opening in front of the bulb through which the light passed to strike the opal glass was 5 x 5 cm., and, when the lower intensity was being used, a neutral-tint filter of that size, standardized to within an error of 1% by the Research Department of the Eastman Kodak Company, and with a transmission value of 1/1000, was placed in a holder in

[1]The author wishes to express her gratitude to Dr. Carl Murchison, Director of the Psychological Laboratory, and to Dr. Clarence Graham, Assistant Professor of Psychology, for permitting her to use the facilities of the Psychology Laboratory at Clark University during the summer of 1933.

front of the opening. The fixation point was a small flash-light bulb run on a dry-cell, with a variable resistance in the circuit, which made it possible to reduce the brightness, so that it was only just readily visible. The bulb was set at the end of a thin vertical post, which was so adjusted that the center of the filament came at the center of the exposure disc.

The part of the apparatus which permitted the determination of the thresholds consisted of an asbestos-lined light-proof box, in which was a 250-watt concentrated-filament, daylight lamp, running on 110 volts D. C., which was kept constant by means of a rheostat. The light rays passed through a glycerin cooling cell, and were brought to a focus on a transparent glass screen in the front of the apparatus by a condensing lens. By means of a calibrated neutral-tint wedge (Eastman Kodak Company Wratten wedge) and its balancing wedge, which were placed directly before the focus of the light beam, close to the experimental device, the intensity of the light could be varied. The wedge, together with the balancing wedge, transmitted from 15.5% of the incident light at one end, to .0775% at the other end. The grosser adjustments in intensity were made by inserting a neutral-tint filter of 1/10 transmission value directly in front of the calibrated wedge. The wedge was controlled by a screw which was operated by hand. Directly in front of this apparatus and 81 cm. from it was the opal glass previously described. Without any wedge or filters the brightness of the opal glass was determined photometrically to be 1.95 millilamberts when illuminated only by the threshold light.

When the thresholds were being determined the size of the exposure field was slightly reduced by dropping a cardboard screen, with an opening 1/4 inch smaller in diameter than the diameter of the exposure disc (subtending an angle of 1.38° at the eye), in front of the exposure field. It also carried a small upright post, slightly larger than the post on which the fixation light was supported. This screen was dropped into place after the eye had been exposed, but before the threshold was determined. This resulted in a slight reduction of the total area for the determination of the threshold and was done so that there would be no danger of a slight slipping of fixation resulting in the threshold determination's being made upon a part of the eye which had not been light-adapted.

III. Procedure

O was seated in the chair with his head in a head-rest, and given a preliminary period of 7 or 8 min. of dark-adaptation. This was considered sufficient, since only the rod-free area was being stimulated. The exposure area was purposely made to subtend a much smaller area than is rod-free in the eye in order to prevent the possibility of a slip in fixation producing rod stimulation. After the dark-adaptation period, O was warned to fixate and, after a "ready-now" signal, the exposure light was turned on; in some series the duration of exposure was 15 sec., in others 30 sec. At the end of the exposure period, the eye was dark-adapted for a definite time (0 sec., 10 sec., 15 sec., 30 sec., and 120 sec. of dark-adaptation were the periods used), and the threshold was then determined with the smaller stimulating area. The method was to set the filter and wedge in such a way that O could not report any sensation, and then gradually to increase the intensity of the light by adjusting the neutral wedge by turning the screw until O said "Now." The first observation in each series was consistently discarded, because E frequently set the threshold light at such a point that 30 or 40 sec. were wasted in finding the threshold, which resulted in a much longer period of adaptation than had been specified. After E had an idea of where the threshold lay, the threshold light could be more accurately adjusted, and was so arranged that O's reports of "Now" would come within 1 or 2 sec. after exposure of the threshold. [With one O this duration was measured with one set of readings and found to vary from 2.0 to 1.2 sec., with the mean at 1.56 sec., but the curves have not be extrapolated. It will be found that the reliabilities of the differences between the curves after the exposure period has been completed, but before dark-adaptation has started, are very satisfactory. Out of 11 comparisons, there are 8 where the chances that the found difference will remain above 0 are 100 out of 100. In the other three cases, the chances are 97, 98, and 98.6 out of 100 that the difference will remain above 0. Extrapolation would not significantly alter the results. (See Table 4.)] Each successive setting, however, was made slightly different in order to prevent O from becoming habituated to saying "Now" after a set interval. When the scale settings on the neutral wedge were in the range below 10 cm. (transmission value of .775%

or more) the number of trials required was adjusted as follows: With a range of .8 cm. or less in the individual threshold determinations, 5 trials (after the first discarded trial) were considered sufficient. One trial was added for each increase of .4 cm. in variability up to 2.4 cm. of variability; for the few cases in which variabliity was 2.5 cm. or greater 10 trials were required. When the scale settings were in the range above 10 cm. (transmission value less than .775%) the numbers of trials were adjusted as follows: With a range of 1 cm. or less in the individual threshold determinations, 5 trials were used. One trial was added for each increase of .5 cm. in variability up to 3.0 cm.; for all cases in which variability was 3.1 cm. or greater 10 trials were required. These arbitrary standards of consistency were selected after analyzing the results of six observers in a similar problem. Between every two threshold determinations enough time was allowed for complete dark-adaptation.

Four individuals served as O's; all had had considerable practice in psychological observations, but all were given at least one hour of training.[2] Every time-intensity relationship was observed by at least three O's, and in one case four were used.

IV. Results

An analysis of the obtained results brings out very clearly the fact that the individual O's have significantly and consistently different thresholds; e.g., it may be seen from Tables 1 and 2 that K's results range between .0073 and .00032 ml., while H's results range between .1507 and .0228 ml.; basically, therefore, K's eyes appear to be about 75 times as sensitive as H's. Because averages obscure such differences, and sometimes even give a false picture of the results, all the data from the present study are reported as of individual O's. In Tables 1 and 2, and Figures 1-5, the data are presented, and their analysis indicates the following points:

1. When the duration of exposure is the same, but the brightnesses of the lights differ, two different effects are evident:

[2]The author wishes to express her gratitude to the following individuals who were so kind as to serve as subjects: Dr. M. Crook (C), Miss H. Hunter (H), Mr. E. H. Kemp (K), and Mr. J. Little (L). Further, I wish gratefully to acknowledge the services of Miss S. Goldman, Miss H. Hunter, Mr. E. H. Kemp, Miss M. Murchison, Mr. P. Murchison, and Mr. L. Riggs, who acted as O's in a long series of preliminary experiments.

TABLE 1

LIGHT-ADAPTATION OF 30 SEC.

Time (sec.)	Intensity 1/1* MC	EK	JL	Time (sec.)	Intensity 1/1000 MC	EK	JL
0	.0817	.00731	.0581	0	.0659	.00617	.01070
10	.0239	.00233	.0148	10	.0218	.00340	.00905
15	.0182	.00183	.00976	15	.0163	.00270	.00638
30	.0134	.00142	.00499	30	.0117	.00180	.00476
120	.0044	.00032	.00345	120	.00375	.00086	.00312

*The figures in each case refer to the threshold value of the light, after the indicated seconds of dark-adaptation, in millilamberts.

TABLE 2

LIGHT-ADAPTATION OF 15 SEC.

Time (sec.)	Intensity 1/1 MC	HH	JL	Time (sec.)	Intensity 1/1000 MC	HH	EK	JL
0	.0688	.1507	.1509	0	.05410	.0858	.00300	.00625
10	.0210	.0712	.0337	10	.01740	.0628	.00130	.00595
15	.0147	.0560	.0175	15	.01120	.0476	.00118	.00375
30	.0106	.0252	.0084	30	.00784	.0393	.00099	.00220
120	.00937	.0228	.0050	120	.00337	.0368	.00043	.00157

a. Sensitivity is decreased more by the brighter light, and the initial increase in sensitivity during the subsequent dark-adaptation is more rapid. (Tables 1 and 2; Figures 1-5.) The chances that the differences in sensitivity following exposure to the brighter and dimmer lights will always be greater than 0 are, with the shorter duration, 100 out of 100 for all three O's; with the longer duration, the chances that the differences will remain greater than 0 are 100 out of 100 for two O's, and 98 out of 100 for the third O. (See Table 4.)

b. It is found that after 30 sec.'s dark-adaptation there is a tendency for all the curves to approach a uniform level, but for two O's (H and K) 120 sec.'s dark-adaptation produces a *lower* threshold after exposure to a *bright* light than it does after exposure to a dim one; for the other two O's (C and L) the reverse is the case. (See Tables 1 and 2; Figures 1-5.)

Analysis of the differences in these cases indicates that they are reliable in 4 of the 6 cases (see Table 5). [It must be stated that

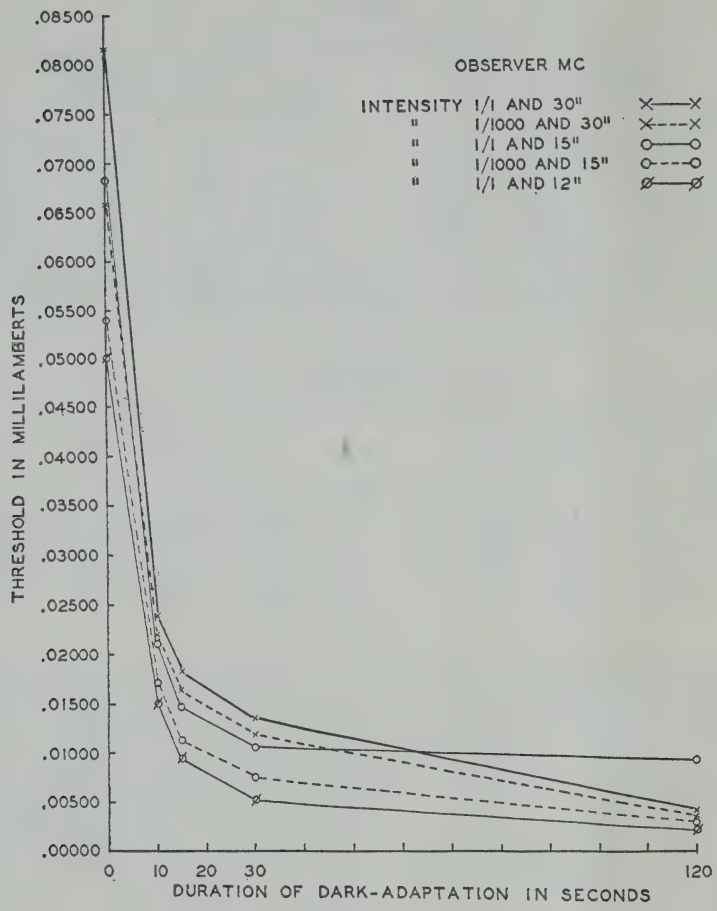

FIGURE 1

the formula used for computing S.D. was $\sqrt{\frac{\Sigma d^2}{N\text{-}1}}$ (1, formula 45, p. 106), instead of the usual $\sqrt{\frac{\Sigma d^2}{N}}$. This formula was used because in no case did the number of determinations exceed 10, and in

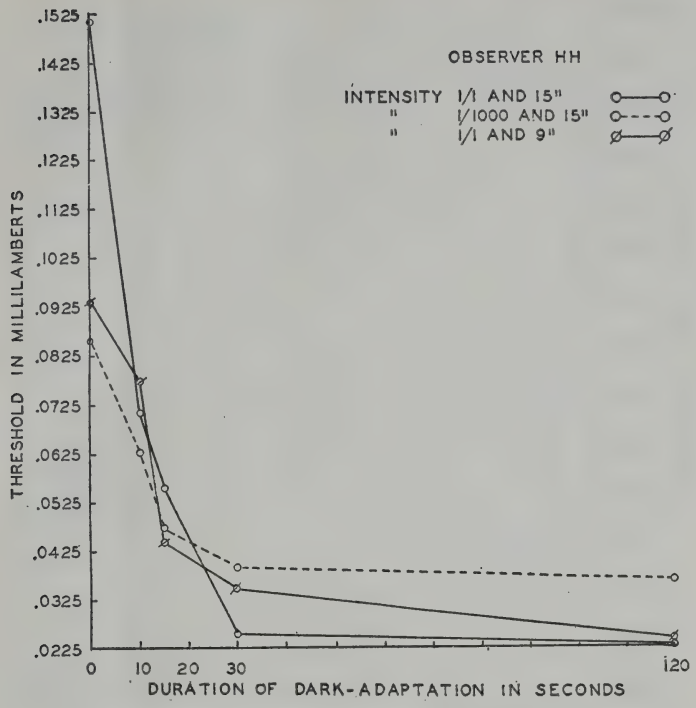

FIGURE 2

most cases there were only 5 or 6. In computing the S.D.$_{mean}$ the formula $\frac{\text{S.D.}_{dist}}{\sqrt{N-3}}$ (1, formula 6, p. 103) was used for the same reason. In this way the determination of the reliabilities was made as conservative as possible. The usual formula for the S.D.$_{diff.}$ $(\sqrt{\sigma^2_{mean_1} + \sigma^2_{mean_2}})$ (2, pp. 129 ff.) was used.]

These data are difficult to interpret, but two factors stand out: (1) Differences in intensity of exposure light have different effects upon individuals; in two cases (H and K) to increase the intensity of the light increases not only the speed of the *initial* dark-adaptation (see *a* above), but the sensitivity continues to increase faster until the curves cross (Figures 2 and 3); after 120 sec. of dark-adaptation

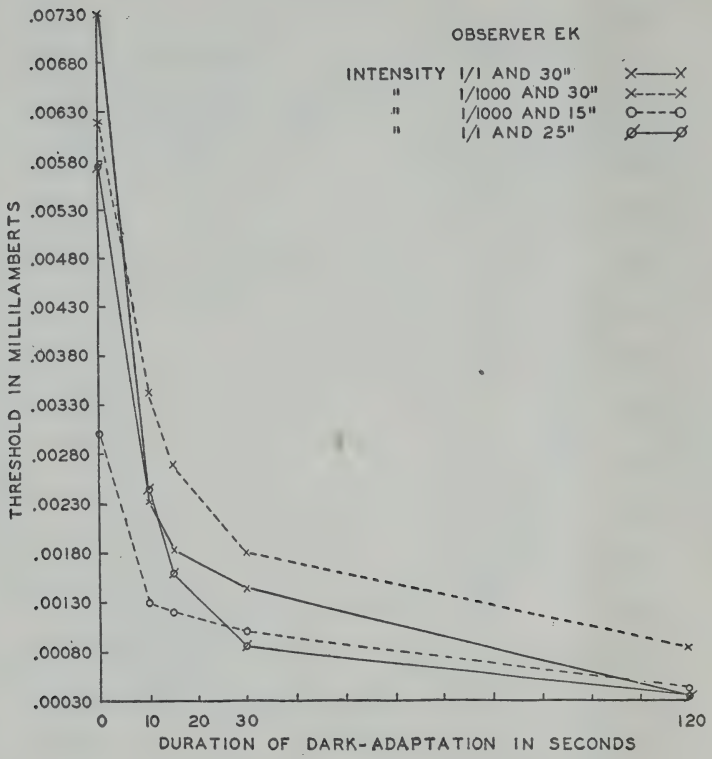

FIGURE 3

the threshold is higher after exposure to the dimmer light, and these results are completely reliable (Table 5). With C and L, on the other hand, although the initial rate at which sensitivity increases is greater after exposure to the brighter light, it soon slows down, so that the curves diagramming the increase in sensitivity never cross, but sensitivity after exposure to the higher intensity remains *less,* even after 120 sec. of dark-adaptation (Figures 1, 4, and 5). (2) When the duration is 30 sec., the results of neither C nor L are reliable (Table 5), but when the duration is 15 sec. both results are completely reliable. This may be interpreted to mean that after the longer duration of exposure the difference between the

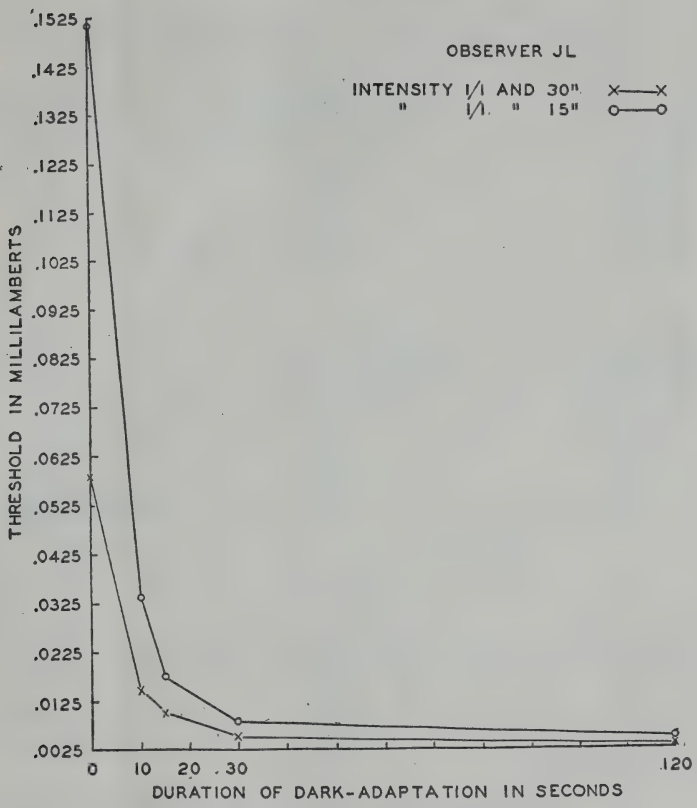

FIGURE 4

intensities is relatively less significant than with the shorter duration, so that after 120 sec.'s dark-adaptation the sensitivity is practically the same.

2. When the intensities of the exposure lights are the same, but the durations of exposure differ, the results are found to depend upon the absolute intensity of the stimulating lights.

a. With the dimmer light (3 O's used, C, K, and L; see Tables 1 and 2; Figures 1, 3, and 5):

1). The sensitivity of the eye shows a more marked decrease following the longer exposure time, and, during the subsequent dark-

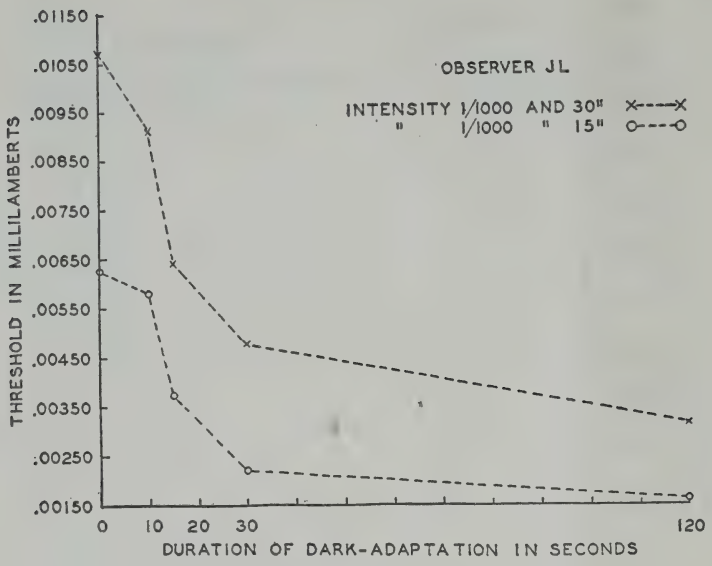

FIGURE 5

adaptation, shows a more rapid initial increase; this is true of the results of all three O's. The chances that there is a real difference in sensitivity at the end of the exposure time before dark-adaptation begins are 100 out of 100 for two O's, and 98.6 out of 100 for the third O (Table 4).

2). In all three cases after 120 sec.'s dark-adaptation, the threshold is *lower* following the 15-sec. period of light-adaptation than following a 30-sec. period. When we analyze these differences we find that two of the three are reliable (Table 5), while the third (C) is practically a chance difference.

b. With the brighter light (2 O's used, C and L; see Tables 1 and 2, Figures 1 and 4):

1). For C, the initial increase in sensitivity during dark-adaptation is more rapid following the *longer* duration of exposure [cf. *a* 1) above], while for L the more rapid increase in sensitivity follows the *shorter* duration of light-adaptation.

2). The threshold in both cases, after 120 sec.'s dark-adaptation, is *lower* after the longer exposure time, and the differences

TABLE 3

THRESHOLDS AFTER DIFFERENT DURATIONS OF DARK-ADAPTATION WHEN THE INITIAL THRESHOLDS ARE APPROXIMATELY THE SAME

OBSERVER C

Time of dark-adaptation (sec.)	Threshold after exposure to 1/1000 for 15 sec.	Threshold after exposure to 1/1 for 12 sec.
0	.05410	.04990
10	.01740	.01512
15	.01120	.00960
30	.00784	.00537
120	.00337	.00239

OBSERVER H

Time of dark-adaptation (sec.)	Threshold after exposure to 1/1000 for 15 sec.	Threshold after exposure to 1/1 for 9 sec.
0	.0858	.0938
10	.0628	.0775
15	.0476	.0447
30	.0393	.0352
120	.0368	.0240

OBSERVER K

Time of dark-adaptation (sec.)	Threshold after exposure to 1/1000 for 30 sec.	Threshold after exposure to 1/1 for 25 sec.
0	.00617	.00572
10	.00340	.00244
15	.00270	.00158
30	.00180	.00086
120	.00086	.00034

in both cases are almost, though not perfectly, reliable. [Cf. *a* 2) above.] (Table 5.)

A peculiarity in the results of L should be noted here. It is found from inspection of the data that after exposure to the higher intensity for 30 sec. the threshold is *lower* than after exposure to the same intensity for only 15 sec., and the chances that this is a true difference are 100 out of 100. (Table 4.) This is the reverse of what is found with C for the same intensity, and with the lower intensity for all the O's, including L himself. In view of this fact, it is perhaps to be expected that the course of dark-adaptation will also be reversed for L, as is indeed the case; with

TABLE 4

RELIABILITY OF THRESHOLD DIFFERENCES AFTER 0 SEC.'S DARK-ADAPTATION

I. EFFECT OF INTENSITY

1. *Shorter duration (15 sec.)*

Observer	Threshold lower	$D/S.D._{diff.}$	Chances $D>0$
C	1/1000	3.94	100 in 100
H	1/1000	4.7	100 in 100
L	1/1000	14.3	100 in 100

2. *Longer duration (30 sec.)*

Observer	Threshold lower	$D/S.D._{diff.}$	Chances $D>0$
C	1/1000	2.02	98 in 100
K	1/1000	5.45	100 in 100
L	1/1000	8.66	100 in 100

II. EFFECT OF DURATION

1. *Lower intensity (1/1000)*

Observer	Threshold lower	$D/S.D._{diff.}$	Chances $D>0$
C	15 sec.	2.22	98.6 in 100
K	15 sec.	11.7	100 in 100
L	15 sec.	8.02	100 in 100

2. *Higher intensity (1/1)*

Observer	Threshold lower	$D/S.D._{diff.}$	Chances $D>0$
C	15 sec.	1.88	97 in 100
L	30 sec.	8.06	100 in 100

III. SPECIAL CASES

	$D/S.D._{diff.}$	Chances $D>0$
1. 1/1 & 12 sec. matched with 1/1000 & 15 sec. (C)—threshold lower after exposure to 1/1	.568	71 in 100
2. 1/1 & 9 sec. matched with 1/1000 & 15 sec. (H)—threshold lower after exposure to 1/1000	.794	79 in 100
3. 1/1 & 25 sec. matched with 1/100 & 30 sec. (K)—threshold lower after exposure to 1/1	1.46	92 in 100

the higher intensity for L, the more rapid increase in sensitivity during dark-adaptation occurs following the *shorter* duration of exposure.

From the above analysis of the effects of the duration of exposure upon the course of the subsequent dark-adaptation, we can conclude that the *longer* the duration of exposure, the *more rapid* the initial increase in sensitivity during dark-adaptation (with one exception),

TABLE 5

RELIABILITY OF THRESHOLD DIFFERENCES AFTER 120 SEC.'S DARK-ADAPTATION

I. EFFECT OF INTENSITY

1. *Short duration (15 sec.)*

Observer	Threshold lower	D/S.D.$_{diff.}$	Chances D>0
C	1/1000	3.41	100 in 100
H	1/1	4.27	100 in 100
L	1/1000	6.18	100 in 100

2. *Long duration (30 sec.)*

Observer	Threshold lower	D/S.D.$_{diff.}$	Chances D>0
C	1/1000	.485	68 in 100
K	1/1	13.6	100 in 100
L	1/1000	.488	69 in 100

II. EFFECT OF DURATION

1. *Lower intensity (1/1000)*

Observer	Threshold lower	D/S.D.$_{diff.}$	Chances D>0
C	15 sec.	.544	71 in 100
K	15 sec.	5.11	100 in 100
L	15 sec.	3.79	100 in 100

2. *Higher intensity (1/1)*

Observer	Threshold lower	D/S.D.$_{diff.}$	Chances D>0
C	30 sec.	2.3	98.9 in 100
L	30 sec.	2.02	98.0 in 100

III. SPECIAL CASES

	D/S.D.$_{diff.}$	Chances D>0
1. 1/1 & 12 sec. matched with 1/1000 & 15 sec. (C)—threshold lower after exposure to 1/1	1.49	93 in 100
2. 1/1 & 9 sec. matched with 1/1000 & 15 sec. (H)—threshold lower after exposure to 1/1	22.4	100 in 100
3. 1/1 & 25 sec. matched with 1/1000 & 30 sec. (K)—threshold lower after exposure to 1/1	9.87	100 in 100

indicating that the effect of added stimulation in terms of time is to increase the rapidity of recovery of sensitivity. The level of sensitivity attained after 120 sec. of dark-adaptation, after exposure to the dimmer light, is lower after the *shorter* duration, while with the brighter light the threshold is lower after the *longer* duration. Of these five differences, two are absolutely reliable, two are practically so, and one is unreliable. (See Table 5.) When we analyze these results it seems clear that a difference in duration has a significantly

different result, depending upon whether the exposure light is bright or dim. With the brighter light, to increase the duration of exposure is to decrease the initial sensitivity of the eye; following this there is, during the initial stage of dark-adaptation, a rapid recovery of sensitivity, and after 120 sec. a lower threshold, indicating that the speed of recovery has continued faster for the long than for the short exposure time. With the dim light, on the other hand, although the initial rapidity is greater after long than after short exposure time, this speed does not continue, and after 120 sec.'s dark-adaptation, the sensitivity is less after the long than after the short exposure time. For C the difference is not reliable, indicating that for him adaptation is practically complete after 120 sec.'s dark-adaptation, and the effect of the exposure differences is hidden.

It is clear from this analysis that the effects of time and intensity are not interchangeable, but it is obvious that increasing the duration of exposure can, to some extent, at least, compensate for a decrease in intensity. In order to determine to what extent this is true, a situation was deliberately set up in which it was attempted to make an increase in intensity compensate for short exposure time in producing the same initial threshold. Having already determined the threshold of sensitivity after exposure to intensity 1/1000 for 15 sec. (C and H) and for 30 sec. (K), the duration of exposure necessary to produce approximately the same threshold with intensity 1/1 was empirically determined. In Table 4 are given the reliabilities of the differences between these pairs of results; it is to be noted that, although exactly the same sensitivity was never obtained, the differences in no case are entirely reliable, although K's are nearly so. Since the initial sensitivity of the eye was approximately the same in these cases, it might be supposed that the course of dark-adaptation would be the same in both cases. This is not the case, however. With all three O's it is found that, although the curves start at the same level, the initial increase in sensitivity during dark-adaptation is more abrupt after exposure to the brighter light (although H's curve is so irregular it is difficult to tell what the initial slope should be), and after 120 sec.'s dark-adaptation the threshold is lower. (See Table 3, Figures 1, 2 and 3.) The differences in two of these cases are reliable, while the third (C) is nearly so. (Table 5.) The apparent inconsistency of these results when compared to those of the regular series, in which four out of six cases

showed the threshold after 120 sec.'s dark-adaptation to be *higher* following exposure to the brighter light, instead of lower, as it is here, may be explained when it is noted that the two exceptions to the rule in the original series are H and K, who give results here consistent with their earlier performance.

We may conclude from these results that the intensity rather than the duration of the exposure light is the more significant factor in producing a rapid initial increase in the sensitivity of the eye during dark-adaptation. It also seems valid to conclude that the greater the stimulation, either in terms of time or intensity, the more rapidly the final threshold of sensitivity is attained.

V. Discussion of Results

The present study was undertaken in order to verify by another experimental technique the results obtained in an earlier study. A matching method was used in the prevous study, the apparent brightness of the exposure field being determined after different periods of dark-adaptation by comparing its brightness with that of a similar field stimulating a comparable area on the other (dark-adapted) eye. This study had also indicated that time and intensity are not interchangeable factors in producing a given level of sensitivity, or in determining the pattern of the subsequent dark-adaptation. When we compare point by point the results obtained in the earlier study with those obtained in the present investigation, we find the following:

1. When the duration of exposure is the same, but the intensities differ:

a. It was found in the earlier study that the initial increase in sensitivity during dark-adaptation was greater for the dimmer light; this was true in 5 out of 6 individual curves, as well as the averages. In the present study the reverse is true, the more rapid initial increase occurring following exposure to the brighter light, and this is true for all cases, with both exposure times. It would appear that the findings in the present study, with respect to intensity, are more consistent with the effects produced by duration than the results obtained in the previous study. We have already made the point that either long duration or high intensity is of high stimulating value. Long duration (high stimulating value) in 4 out of 5 cases produced

a more rapid initial increase in sensitivity than short duration (low stimulating value). We would, therefore, expect high intensity to produce a more rapid initial increase than low intensity; this is true in the present study, but not in the earlier one. The internal consistency of the results in both experiments leads one to think, however, that the explanation must lie in the experimental method.

b. The previous study indicated that, when the duration of exposure was the same, a longer time was required for complete dark-adaptation after exposure to a brighter light; i.e., when the exposure light was bright, 120 sec.'s dark-adaptation leaves the eye *less* sensitive than if the exposure light was a dim one. This was true for all 6 of the individual comparisons as well as for the averages.

In the present experiment rather inconsistent results were obtained, for with the longer exposure time (30 sec.), 120 sec.'s dark-adaptation resulted once (K) in leaving the eye *less* sensitive after exposure to the *dim* light than after exposure to the bright one, and twice (C and L) in leaving the eye *less* sensitive after exposure to the *bright* light than after exposure to the dim one. With the shorter exposure time, 120 sec.'s dark-adaptation resulted once (H) in leaving the eye *less* sensitive after exposure to the *dim* light, and twice (C and L) in leaving it *less* sensitive after exposure to the *bright* light. These results indicate that some individual factor is basic to this condition. Of these results, four (C and L for both durations of exposure) are in agreement with the earlier experiment, while two disagree (H and K). Both of the latter are completely reliable differences, while two of the former are, and two are not. (Table 5.) A possible explanation of the discrepancy, aside from individual differences and the difference in the methods used, may lie in the fact that the brighter light in the first study was about 10 times as bright as the brighter light in the present study. It had been thought that this difference would be immaterial, since the ratio between the brighter and dimmer lights was the same in both cases. It is possible, however, that the absolute intensity has some effect upon the relative results.

2. When the intensities of the lights are the same, but the duration of the exposure differs:

a. It was found in the first study that the initial rate of increase in sensitivity during dark-adaptation was *greater* for the

longer durations. This result was confirmed in the present study in 4 out of 5 of the individual curves, the single exception being L's result from the higher intensity.

b. The difference in duration in the previous experiment produced different end results for the two intensities.

1). With the lower intensity, the duration of exposure has little effect, since dark-adaptation is practically complete for both in 30 sec. If there is any difference, it is for the 15-sec. exposure to require slightly longer for complete adaptation.

2). With the higher intensity, the sensitivity of the eye after 120 sec.'s dark-adaptation was *less* following 30 sec.'s exposure than following the 15-sec. exposure.

In the present experiment, the absolute intensity of the exposure light also affects the speed with which the dark-adaptation is completed.

1). With the lower intensity, the threshold after 120 sec.'s dark-adaptation is *higher* after 30 sec.'s exposure than after 15 sec.'s exposure. This is true of all three O's (C, K, and L).

2). With the higher intensity, an increase in the duration of exposure results in a *lower* threshold after 120 sec.'s dark-adaptation, i.e., after 120 sec.'s dark-adaptation, sensitivity is *greater* after 30 sec.'s exposure than after 15 sec.'s exposure. This is true for both C and L.

Comparing the results of the first experiment with those of the present study with respect to the effect of differences in duration, we can say that for both intensities the results are diametrically opposed. It must be pointed out that these results in the first study, showing clearly in the averages, could be confirmed with only one O who observed under all four conditions. The reliability of the differences in the present study has already been discussed. It is shown that for the lower intensity two differences are completely reliable, while one is unreliable; for the higher intensity both differences are practically reliable. (See Table 5.) The consistency of direction as well as the reliability figures indicates the presence of a real difference. The difference between the two studies is difficult to explain. Aside from the difference in method, which may be the significant factor, we may point to the rather considerable individual differences, which possibly may be the most reasonable explanation.

Since both high intensity and long duration (with one exception)

produce low initial sensitivity, we may consider them to be of high stimulating value. The results obtained from the two experiments are summarized in tabular form in Table 6 in order to indicate the

TABLE 6

Experiment I	Experiment II
1. *Long time* produces a rapid initial dark-adaptation (no exceptions)	1. *Long time* produces a rapid initial dark-adaptation (1 exception)
2. *High intensity* produces slow initial dark-adaptation (5 out of 6 cases)	2. *High intensity* produces a rapid initial dark-adaptation (no exceptions)
3. *Long time* produces (after 120 sec.'s dark-adaptation) a higher threshold (2 out of 4 cases)	3. *Long time* produces (after 120 sec.'s dark-adaptation) a higher threshold (3 out of 5 cases)
4. *High intensity* produces (after 120 sec.'s dark-adaptation) a higher threshold (no exceptions)	4. *High intensity* produces (after 120 sec.'s dark-adaptation) a higher threshold (4 out of 6 cases)

relationships between time and intensity in determining the course of the subsequent dark-adaptation. Reference to Table 6 shows us that the chief discrepancy between the two studies lies in the fact that in the present study stimulation by the higher intensity resulted in a *faster* initial dark-adaptation than stimulation by the lower intensity, which is precisely the reverse of the result obtained in the first experiment.

The reason for this discrepancy and for the reversal of the effect of duration upon the final sensitivity (which does not appear in the summarized Table 6) is difficult to ascertain. Besides the difference in the methods used, there are two factors which might possibly have influenced the results. (1) In the first experiment the exposure field was a semicircle; the whole circle would have subtended a visual angle of 3° 12′. The actual stimulating area, therefore, subtended a visual angle of 3° 12′ in a vertical direction, and of 1° 36′ in a horizontal direction. In the second experiment the stimulus field was circular and was made comparable to the smaller of the two directions of the first stimulus; it subtended an angle of 1° 28′. The total area of the field in the first experiment was .00122 steradians, and in the second experiment, .000507 steradians. Both stimulus fields lie well within the rod-free area of the eye.

(2) The higher intensity in Experiment I was approximately 10 times as bright as the higher intensity in Experiment II. This was due to the apparatus which was arranged in such a manner as to make it difficult to obtain high stimulating values. It was felt to be a relatively unimportant matter, however, since the ratio between the higher and lower intensities remained identical. Since the studies agree on a majority of points, it seems improbable that these two factors could have produced the discrepancies noted. The lower absolute stimulating value of both intensities in the present study may perhaps explain the inconsistencies in final threshold level, since 120 sec. in most cases was almost adequate to produce practically complete dark-adaptation, which was not the case with the higher intensity in the first study.

Attention should be called also to the fact that throughout both experiments individual differences between O's are of considerable size and possibly play a significant part in producing the differences which appear between the two studies.

VI. Conclusions

The following conclusions may be drawn from the reported data:

1. In general, long duration and high intensity both have high stimulating value, as proved by the rapid reduction in sensitivity produced by exposure, but they are not effective to the same degree.
2. The course of dark-adaptation is dependent upon the manner in which the particular degree of sensitivity of the eye, at the start of dark-adaptation, has been produced.
3. Much stimulation, regardless of whether in terms of high intensity or long duration, produces a rapid initial increase in the sensitivity of the eye during the subsequent dark-adaptation, but to some extent this appears to be a function of the method by which the sensitivity is measured.
4. Much stimulation tends to produce a higher threshold after 120 sec.'s dark-adaptation, i.e., a longer time is required for complete dark-adaptation, but the particular time-intensity relationship which produced the initial sensitivity is an important factor.
5. In general, we may conclude that the threshold and matching methods of measuring the sensitivity of the eye produce similar but not identical results.

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LA DURÉE ET L'INTENSITÉ DE LA LUMIÈRE EXPOSÉE COMME FACTEURS DANS LA DÉTERMINATION DU COURS DE L'ADAPTATION SUBSÉQUENTE À L'OBSCURITÉ: II. LA MÉTHODE DU SEUIL

(Résumé)

On a fait cette étude dans le but de déterminer si les résultats obtenus dans une expérience antérieure, qui a employé une méthode de comparaison pour mesurer la sensitivité de l'oeil, seraient les mêmes quand on se servait de la technique du seuil. On a stimulé l'oeil en l'exposant pour une durée définie à une lumière d'une clarté définie; on a ensuite déterminé sa sensitivité par la technique du seuil après diverses durées de l'adaptation à l'obscurité. On a étudié quatre relations de la durée et de l'intensité. Les résultats indiquent que: (1) Beaucoup de stimulation, en termes ou de la durée ou de l'intensité de la lumière exposée, cause une décroissement marqué de la sensitivité de l'oeil, mais la vitesse initiale de l'accroissement de la sensitivité pendant l'adaptation subséquente à l'obscurité est plus rapide que si l'oeil a été exposé antérieurement à moins de stimulation. (2) L'adaptation à l'obscurité est complétée plus lentement après beaucoup de stimulation qu'après peu de stimulation. (3) Les différences entre les O's individuels sont marqués. (4) Quoique la durée et l'intensité puissent être un peu compensables mutuellement, elles ne sont pas des facteurs qui peuvent être changés l'un pour l'autre, et le cours de l'adaptation à l'obscurité dépend de la relation spéciale de la durée et de l'intensité laquelle produit la sensitivité initiale. (5) Les résultats obtenus dans cette étude ressemblent mais ne sont pas identiques avec ceux obtenus dans l'étude antérieure.

Johannsen

DIE DAUER UND INTENSITÄT DES EXPOSITIONSLICHTES ALS FAKTOREN IN DER BESTIMMUNG DES VERLAUFS DER NACHFOLGENDEN DUNKELADAPTION: II. SCHWELLENMETHODE

(Referat)

Die vorliegende Untersuchung wurde unternommen, um zu bestimmen, ob die Ergebnisse, die in einem früheren Experiment erreicht wurden, das zur Messung der Augenempfindlichkeit eine Paarungsmethode gebrauchte, wiederholt würden, falls die Schwellentechnik angewandt wird. Das Auge wurde dadurch gereizt, dass es auf die Dauer einer bestimmten Periode einem Licht bestimmter Helligkeit ausgesetzt wurde; dann wurde desen Empfindlichkeit nach verschieden langen Zeitabschnitten der Dunkeladaption durch die Schwellentechnik bestimmt. Man untersuchte vier Zeitstärkebeziehungen. Die Ergebnisse sind die folgenden: 1.) Viel Reizung im Sinne der Dauer oder der Intensität des Expositionslichtes ergibt eine ausgeprägte Abnahme in der Augenempfindlichkeit, aber die anfängliche Zunahme der Empfindlichkeit während der nachfolgenden Dunkeladaption ist schneller, als bei vorangehender geringeren Exposition des Auges. 2.) Die Dunkeladaption wird langsamer vollendet nach viel als nach wenig Reizung. 3.) Unterschiede zwischen den Beobachtungen der Individuen ist ausgeprägt. 4.) Obschon Zeit und Stärke einander bis zu einem gewissen Grad kompensieren können, sind sie doch keine vertauschbaren Faktoren, und der Verlauf der Dunkeladaption hängt von der besonderen Zeitstärkebeziehung ab, die die anfängliche Empfindlichkeit hervorruft. 5.) Die durch diese Untersuchung erhaltenen Ergebnisse sind denen in der früheren Untersuchung erhaltenen ähnlich, sind aber mit ihnen nicht identisch.

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THE SPECIFICITY OF FACIAL EXPRESSIONS*

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Among the many studies of the relation between emotional reactions and facial expressions, the well-known experiment by Landis (1) seems to be the only one in which a systematic analysis of actual responses to emotional stimuli has been attempted. It is rightly considered, therefore, as a study of outstanding importance. Careful examination of the work, however, reveals a statistical misinterpretation, correction of which reverses or changes very substantially the major conclusions of the paper.

Landis' method, it will be recalled, was to outline with charcoal on the subject's face the regions of the principal muscles and muscle groups. A series of 17 emotional stimuli listed below was given each subject.

Situations

1. Popular music—three jazz records
2. Technical music—violin solo records
3. The Bible—Luke 6: 18-49

4L Falsehood
4T Truth

5. Ammonia, under label "Syrup of Lemon"
6. Shot—firecracker
7. *Faux pas*—reading of subject's account of his social error
9. Ten pictures of skin diseases
10. Mental multiplication with noise distraction
11. Pornographic pictures
12. Photographs of artists' models
13. Sex case histories read by subject from Havelock Ellis
14. Feeling of live frogs in bucket and receiving shock
15. Decapitating live rat
16. Mental multiplication with electric shock distraction
17. Relief—threat of other stimuli followed by announcement that there were no more

During the response in each case, a photograph of the subject's face was made. The subject's report was taken after each reaction. The photographs were compared with similar pictures of the subject

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taken in a resting condition to determine which muscles were active. Landis found it impossible to measure the amount of activity of the muscles in the pictures, but judged "involvement" or "non-involvement" in each case. He also made estimates of degree of involvement, but did not regard them as reliable enough to use in his main development. The data, therefore, take the form of percentage of cases in which the contraction of the various muscles was involved. Percentage, or frequency of involvement, was given for each muscle in each situation, for each muscle for each individual, and for each muscle in various reported emotional conditions. The tables giving the data classified by situation and by subject's report are reproduced here (Table 1 and Table 2). Landis' next step was to compute the average involvement of each muscle in all situations (figures given in the columns at the right in the reproduced tables); then to apply a statistical criterion to determine when the muscle was involved to a frequency which was reliably different from its own average involvement. In only a few cases did the figures satisfy the criterion; and in these special examination convinced Landis that the differences were not significant either. In conclusion, therefore, he states:

> "In no situation was the contraction of a muscle, a group of muscles, or expression found to occur with sufficient frequency as to be significant. There is no expression typical of any situation in this experiment."
>
> "With no verbal report of a given emotion did a muscle, group of muscles, or expression occur with sufficient frequency to be considered characteristic of the emotion. There is no expression typically associated with any verbal report."

Considering the responses classified by subjects, Landis concluded that there were responses sometimes typical of individuals.

It is with the statistical criterion used in arriving at these conclusions that serious fault is to be found. Landis explained it as follows:

> "In many statistical problems it has been found that difference between averages that is three or more times the probable error of the difference is significant. A modification of the plan was tried on these data. That is, any figure which deviated from the average of the figures of which it was one, by three or more times the probable error was considered significant. The application of this standard to the data gave results which agree with the (experimenter's?) subjective judgment of the original data and which are consistent with themselves."

TABLE 1

PERCENTAGE OF SUBJECTS SHOWING INVOLVEMENT OF MUSCLES IN VARIOUS SITUATIONS
(Slightly abridged from Landis)

	Situations																			
	1	2	3	4T	4L	5	6	7	9	10	11	12	13	14	15	16	17	Av.	M.V.	P.E.
Head right	24	34	9	34	60	11	68	42	11	19	11	12	8	62	25	27	55	30	15.8	13.4
Head left	10	6	6	3	3	15	11	29	17	20	18	10	4	12	25	29	9	13	6.3	5.4
Head back	12	8	6	27	16	0	11	11	0	18	0	5	2	26	7	14	0	9	6.8	5.0
Head front	36	44	72	10	38	63	29	33	70	49	55	56	63	35	48	38	36	46	14.2	12.0
Frontalis	26	24	22	13	30	26	36	11	15	24	18	27	19	29	25	38	27	24	5.9	5.0
Corrugators	4	10	9	3	3	37	7	15	21	22	11	19	25	12	29	22	0	15	8.6	7.3
U. orb. oc.	36	22	34	17	24	56	46	49	51	46	59	44	54	47	64	54	45	44	10.2	8.6
L. orb. oc.	58	42	63	23	73	70	68	77	77	81	84	76	83	80	78	82	90	71	12.0	10.2
Nasalis	6	0	6	7	5	64	43	11	43	24	40	19	25	29	41	16	9	23	14.9	12.6
Quadrates	6	20	31	33	57	74	64	44	72	73	80	44	71	94	77	73	73	58	20.1	17.0
Zygomaticus	28	16	28	30	51	41	68	51	34	70	65	34	46	74	77	58	82	50	17.0	14.4
Risorius	26	16	16	27	38	37	66	45	26	45	43	37	35	77	79	43	73	37	14.8	12.5
Lips open	26	8	12	37	57	30	54	35	24	78	35	37	20	77	66	73	64	43	19.7	16.7
Lips pursed	16	10	6	3	13	4	7	9	8	5	11	0	6	12	0	13	0	7	4.4	3.7
Mentalis	6	4	6	7	5	11	3	2	2	7	0	0	0	3	7	2	9	4	2.8	2.4
Triangularis	44	52	59	47	32	22	32	27	28	20	40	27	42	21	25	46	46	36	10.6	8.4
Eyes closed	14	28	3	10	30	23	14	16	2	16	3	0	4	15	20	18	0	13	7.9	6.7
Eyes wide	22	12	0	13	16	4	3	13	2	11	3	7	8	21	2	15	18	10	5.9	5.0
Eyes up	12	2	0	13	5	0	3	2	4	7	3	0	0	3	2	2	0	3	2.8	2.4
Bite lip	2	0	0	0	3	0	0	4	0	3	0	0	2	3	5	2	0	1	1.3	1.1
Chin back	4	4	0	0	6	15	0	2	4	0	3	5	4	0	2	0	0	3	2.3	1.9
Tension	20	20	9	27	57	52	82	44	64	74	42	41	50	85	89	44	36	49	18.8	15.9
Relaxation	34	42	12	6	5	11	7	0	0	1	0	2	0	0	9	16	27	17	13.2	11.2
Smile	14	20	12	17	22	26	57	33	23	46	40	29	31	74	52	28	36	33	12.7	10.7

TABLE 2

PERCENTAGE OF SUBJECTS SHOWING INVOLVEMENT OF MUSCLES ACCOMPANIED BY CERTAIN SUBJECTS' REPORTS
(Slightly abridged from Landis)

	Pain I	Dis-gust II	Exas-pera-tion III	Anger IV	Revolt-ing V	Crying VI	Sur-prise VII	Sex VIII	Av.	M.V.	P.E.
Head right	21	10	44	17	8	24	60	6	24	14.2	12.0
Head left	14	13	17	17	20	8	8	17	14	3.5	3.0
Head back	29	3	30	11	4	12	12	3	13	8.3	7.0
Head front	36	70	17	50	68	36	40	57	47	14.5	12.3
Frontalis	43	20	48	28	32	36	36	31	34	6.5	5.5
Corrugators	50	20	26	28	40	20	20	3	26	9.9	8.3
U. orb. oc.	57	67	48	45	44	32	44	37	47	7.8	6.6
L. orb. oc.	78	87	78	78	80	48	76	77	75	6.9	5.8
Nasalis	36	53	35	28	72	4	52	35	39	14.7	12.4
Quadrates	71	60	65	61	80	48	84	63	66	8.6	7.3
Zygomaticus	57	36	61	50	48	24	68	63	51	11.2	9.5
Risorius	21	30	57	33	40	16	72	52	40	15.1	12.7
Lips open	71	37	100	50	28	48	60	43	55	16.8	14.2
Lips pursed	14	17	4	17	0	12	8	0	9	8.0	5.1
Mentalis	0	0	0	4	0	0	0	12	2	3.0	2.5
Triangularis	43	40	35	50	16	32	24	11	36	14.8	12.5
Eyes closed	21	3	13	22	16	48	20	3	18	9.5	8.0
Eyes wide	14	3	9	17	4	8	20	6	10	5.1	4.3
Eyes up	7	0	4	6	0	12	4	0	4	3.1	2.5
Bite lips	0	0	0	6	0	0	0	3	1	1.7	1.4
Chin back	0	7	0	0	0	0	0	3	1	1.8	1.6
Tension	71	60	61	72	80	32	80	66	65	10.7	9.0
Relaxation	21	0	4	11	0	32	0	6	9	9.6	8.1
Smile	7	17	39	11	20	8	60	60	28	18.9	16.0

The last two considerations are undoubtedly correct, but not weighty. The resemblance between Landis' procedure and the standard statistical method to which he refers is very superficial. The standard method is concerned with the difference between the averages of two distributions; Landis', with the deviation of a measure from its own average. The probable error of the standard procedure is the probable error of the difference in question, i.e., it is a measure of its reliability; the probable error in Landis' procedure is the probable error of the distribution, i.e., it is a measure of dispersion. The factor of chance in a distribution is in no way indicated by the magnitude of any measure of dispersion, unless it be proved that all deviations from the average are due to chance, which is the question at issue. The reliability of a figure cannot be determined by simple reference to the probable error of its distribution. The faultiness of the criterion is apparent when it is applied to stature, since it would lead one to assert that a man is not significantly taller than five feet seven inches unless he is at least six feet four.

In fact, the number of "significant" figures revealed by this method in Landis' tables could have been approximately predicted in advance from a table of the normal probability function. In a normal curve 4.3% of the population is beyond the limits of 3 P.E. In Landis' Table 1, 2.7% of the figures fulfill the criterion; in Table 2, 2.6%.

Some other statistical treatment is evidently called for. One technique for determining the chance factor in the measures would be to obtain a coefficient of reliability and compute the probable error of estimate. But the necessary data are not available, as the experiment was not repeated, and probably could not be. The search for the significance of each particular figure in the tables must, therefore, be abandoned. A more general technique may be applied, and possibly may yield the crucial information.

Three hypotheses regarding the nature of the data may be distinguished:

1. There is no consistency to the data at all. Each figure in the table is entirely the product of chance factors.

2. There is a single pattern of facial response which is evoked by any kind of emotional situation, and varies only in degree of contraction. All muscles contract in the same relative proportion.

3. There is a tendency for a number of different facial response patterns to be evoked by different stimuli. These patterns may have much in common, or nothing.

A statistical method which will decide between these three possibilities is required, and correlations between the columns would seem to serve the purpose. If contractions of muscles were entirely random the numerical profile of one expression would bear no resemblance to that of another and would show zero correlation. If there were one expression in varying degrees in the different situations, the correlations should be equal to 1.00. Should there be a series of specific expressions without common elements, the correlations would again be zero, on the average, with the probability of some negative coefficients. But if there were specific reactions with common elements, common to all expressions, or to some only, the correlation coefficients should lie between zero and 1.00. Naturally a certain amount of chance variation should be expected to occur in conjunction with any of these non-chance conditions.

Because of the above considerations corrected rank-difference correlations between the columns of Tables 1 and 2 were computed. Any scheme of analysis would have been more satisfactory if the numbers in the tables represented quantities, rather than simply frequencies, but it is worth while to see where the present figures lead. In computing the correlations consideration was restricted mainly to figures representing the action of specific muscle groups. The estimates of tension and relaxation were included as they do not seem to be related very closely either to each other or to the other figures of the tables, and hence provide new information. It was necessary for a satisfactory use of the correlation technique to eliminate zeros and near zeros when they occur in all columns. It is evident that this is necessary because correlations might be raised to almost any desired height by including muscle groups which did not participate in any reaction. On this account four muscles or groups were left out of consideration in computing the correlations, the mentalis, "eyes up," "biting lip," and "chin back." All were involved, on the average, less than 5% of the time, and all had a considerable number of zero frequencies in the various situations. Of course, there are quite a number of isolated zero responses of other muscles included in consideration, as they deserve to be. Table 3 gives the correlations between the responses classified, as in Table

TABLE 3
INTERCORRELATIONS BETWEEN EXPRESSIONS EVOKED BY VARIOUS SITUATIONS

	1	2	3	4L	4T	5	6	7	9	10	11	12	13	14	15	16	17
1		.70	.70	.49	.37	.13	.26	.37	.28	.27	.43	.45	.37	.20	.21	.49	.60
2			.61	.49	.36	.15	.19	.27	.35	.03	.23	.33	.38	.14	.09	.32	.42
3				.56	.45	.61	.57	.58	.77	.65	.73	.78	.76	.51	.59	.77	.79
4L					.78	.44	.77	.75	.55	.64	.58	.67	.61	.83	.63	.73	.82
4T						.17	.70	.63	.34	.48	.54	.56	.42	.72	.47	.64	.75
5							.60	.63	.92	.83	.87	.84	.86	.66	.84	.71	.55
6								.81	.67	.75	.82	.67	.74	.89	.84	.79	.80
7									.82	.79	.81	.70	.77	.81	.85	.84	.83
9										.87	.95	.93	.95	.71	.85	.79	.66
10											.86	.93	.84	.87	.92	.86	.70
11												.91	.94	.76	.86	.85	.77
12													.94	.83	.91	.86	.77
13														.74	.82	.78	.71
14															.89	.76	.82
15																.83	.76
16																	.85
17																	

TABLE 4
INTERCORRELATIONS OF EXPRESSIONS ACCOMPANIED BY VARIOUS SUBJECTS' REPORTS

	I	II	III	IV	V	VI	VII	VIII
I		.81	.77	.86	.94	.80	.61	.43
II			.81	.86	.71	.40	.73	.48
III				.72	.71	.48	.88	.56
IV					.83	.64	.72	.49
V						.34	.77	.58
VI							.35	.44
VII								.61
VIII								

1, by situation. Table 4 gives the correlations between the responses classified in Table 2, by subjects' reports. The figures of Table 4 are less satisfactory than those of Table 3 because Landis' corresponding table is based on fewer subjects, and there are many tie ranks involved in comparing these. But the trend of the two tables is the same.

The following observations may be made from them:

1. Coefficients in Table 3 range from .03 to .95. In Table 4 the range is from .34 to .88.

2. The average of the coefficients in Table 3 is .633. The average in Table 4 is .648.

3. The coefficients are distributed as follows:

No. of coefficients	0-10	10-20	20-30	30-40	40-50	50-60	60-70	70-80	80-90	90-100
Table 3	2	5	7	9	11	10	16	33	33	9
Table 4				2	6	2	3	8	7	

Certain situations and certain verbal reports show low correlations with practically all others, while other situations and verbal reports have high correlations with practically all others.

Which of the three hypotheses, then, do the correlations confirm? Considering only the average coefficient for the moment, can it reasonably be regarded as a chance deviation from 0, according to hypothesis number one, or a chance deviation from 1.00, according to hypothesis number two? Considering the averages as single correlations, the probable errors are, for Table 3, .095, and for Table 4, .092. To interpret these averages as chance deviations from either 0 or 1.00 would evidently be unjustified. It is possible that some of the coefficients are chance deviations from 1.00, although the difference in relation to the P.E. of the coefficient is always substantial. It is perhaps more likely that some are chance deviations from 0, although doubtless some significance ought to be attached to the fact that all figures are positive. But if *any* coefficients deviate significantly from 0 or from 1.00 then the first and second hypotheses are impossible, and here we have a considerable number which lie somewhere between 0 and 1.00.

It is therefore necessary that some form of the third hypothesis be adopted; that is, there are reaction patterns specific to situations and to verbal reports, with common elements of greater or less extent. Some reaction patterns found resemble others, but certainly there is no high degree of resemblance running through all the responses. This conclusion is closer to common notions than is that of Landis, and seems also to fit the primary data better. For it is difficult to regard, as Landis does, 7% smiling in reported pain as not significantly different from 60% smiling in reported sex, 4% involvement of the nasalis in crying as not significantly different from 52% involvement in reported pain, 3% involvement of the corrugators in reported sex as not significantly different from 50% in reported pain, etc.

This conclusion applies to the group of photographs used. It

is probable that it applies to individuals also, but the data do not make the application a certainty.

In using the term "specific" in the preceding discussion, it is intended to refer only to the facts at hand; saying that an expression is specific to a situation means that it differs in pattern from one evoked by some other situation of the experiment. It is not meant to imply that the human visage has a dozen or so different responses corresponding to common emotional titles. Such may be the case, but there is another possibility which seems rather more plausible. It is possible that there is a continuous gradation from one so-called type response to another. Any "type" expression would then be only a point in a continuum. The gradations referred to are, of course, variations in pattern, and might be called qualitative as distinguished from quantitative gradations of intensity of response.

Two further questions may receive an answer from the data: (1) What particular situations resemble each other most or differ most? (2) What muscles are most differential in their responses?

An answer to the first question may be derived from an examination of the correlation coefficients. In Table 3 it may be observed that there is a tendency for the later situations to correlate to a higher degree than the earlier. This fact may indicate a tendency for the reaction to become stereotyped to a certain degree as stimulation is continued. A further influence of a preceding situation upon a later may be evidenced by the fact that adjacent situations tend to

TABLE 5

AVERAGE OF CORRELATIONS INVOLVING PARTICULAR SITUATIONS AND VERBAL REPORTS

Situation	Av. cor.	Situation	Av. cor.	Situation	Av. cor.
1	.408	5	.614	11	.744
2	.316	6	.680	12	.755
3	.652	7	.704	13	.727
4L	.646	9	.716	14	.696
4T	.524	10	.706	15	.710
				16	.741
				17	.726

Verbal report	Av. cor.	Verbal report	Av. cor.
1	.717	5	.689
2	.686	6	.493
3	.704	7	.669
4	.731	8	.513

correlate with each other better than average. Examining the average of all correlations in which any particular situation or verbal report is involved should give information as to their degree of resemblance. These are tabulated in Table 5.

It is understood, of course, that these figures have no significance outside this experiment, since an average could be influenced very largely by the inclusion of a greater or less number of similar stimuli in the series.

Situation 2, listening to classical music, appears to have the most distinctive stimulus properties. Its reactions correlate very little with others, the highest correlation being with the reaction to situation 1, listening to jazz music.

Situation 1 itself (jazz music) is the next most specific in reaction-evoking properties. Its reaction resembles those produced by (2) classical music, by (3) reading the Bible, and by (17) relief.

There is special interest in situations 4L and 4T, lying and truth-telling. Between their responses there is a moderate correlation, but when compared with other responses 4F always shows the lower correlation of the two. Considering the stimuli, it may be observed that 4L resembles most of the others in being an uncomfortable situation for the subjects.

A group of very high correlations is found between situations 11, 12, and 13. These are all stimuli related to sex.

Some less-expected high relationships are found between 9 (skin-disease pictures) and 11, 12, 13 (sex pictures and sex histories), between 10, 12 (multiplication with distraction, and sex photographs) and 15 (decapitating rat).

The results classified by subjects' reports are somewhat easier to interpret. Reports 6 and 8, crying and sex, have decidedly lower average correlations than the other situations. These two also have a very small correlation with each other. Evidently each contains some special feature not common to the other. Also, pain, disgust, exasperation, anger, revolting, and surprise have a good deal in common.

To determine the number and magnitude of factors involved, a tetrad difference or similar technique might be applied to such correlation tables. But with the data at hand it would hardly be justified, as the scores represent only frequencies.

For the purpose of examining the part played by the various

TABLE 6
AVERAGES AND S.D.'S OF RANKS OF MUSCLE GROUPS
From Table 1 (Situations)

	Muscle	Av. rank	S.D.
Head right	1	8.6	4.3
Head left	2	13.0	1.8
Head back	3	14.0	3.3
Head front	4	5.4	4.3
Frontalis	5	9.8	2.8
Corrugators	6	11.5	4.7
U. orb. oculi	7	4.4	2.8
L. orb. oculi	8	1.8	2.2
Nasalis	9	11.5	5.1
Quadrates	10	4.1	3.9
Risorius	11	4.7	2.5
Lips open	12	5.8	2.9
Lips pursed	13	5.8	4.5
Triangularis	14	14.9	3.2
Eyes closed	15	7.2	6.1
Eyes open wide	16	12.5	2.9
Bite lips	17	13.7	2.5
Tension	18	4.6	2.6
Relaxation	19	13.0	4.7
Smile	20	8.3	1.2

TABLE 7
AVERAGES AND S.D.'S OF RANKS OF MUSCLE GROUPS FROM TABLE 2
(BY SUBJECTS' REPORTS)

Muscle	Av. rank	S.D.
1	11.4	4.9
2	16.4	2.0
3	16.3	2.4
4	7.5	2.9
5	9.0	1.9
6	12.3	3.3
7	7.6	2.4
8	1.7	0.6
9	10.3	4.4
10	2.9	0.5
11	5.2	2.8
12	9.1	3.4
13	5.8	3.2
14	15.4	3.3
15	10.2	2.6
16	12.7	5.6
17	16.9	1.3
18	3.5	0.9
19	15.3	4.5
20	13.1	5.3

muscles and muscle groups in expressions, Tables 6 and 7 have been prepared. If there were perfect correlation between expressions under all circumstances, the rank assigned a muscle in any expression's distribution would be the same. The more a muscle's rank shifts from one expression to another, the more specific is its reaction to certain situations in the experiment. Hence the S.D. of the ranks assigned to each muscle was computed. This should be regarded as a measure of the specificity or generality of a muscle's reactions, a small S.D. indicating that the muscle usually reacts in an amount proportional to the intensity of the whole reaction, a large S.D. indicating that its prominence in some expressions is much greater than in others.

From the tables: Highly specific reactions as regards situations are closing the eyes, contraction of the nasalis, general relaxation, contraction of the corrugators, pursing the lips, turning the head to the right and to the front. To what these reactions are especially specific may be seen from Table 3. Closing the eyes, for example, is found in situations 1, 2, 3, 4T, 16, and 17; contraction of the corrugators is greatest in 15, 9, 10, and 3; contraction of the nasalis in 5, 9, 7, 15, etc. The most general reactions are turning the head to the left, contraction of the L. orb. oculi, contraction of the risorius, and biting the lips.

Regarding the results classified by subject's report, the most specific are opening the eyes wide, smiling, turning the head to the right, general relaxation, contraction of the nasalis. Very general reactions so classified are contraction of the L. orb. oculi and contraction of the quadrates, biting the lips, contraction of the frontalis.

Smiling occupies a peculiar status, being general to situations, and specific to subjects' reports. This fact may indicate that significant variations in response are missed when responses are classified by situations: the stimulus error is present. There are other less striking disagreements between the two lists, but considerable divergence might be expected, as only a part of the reactions represented in the table by situations is represented in the table by verbal reports.

If these S.D. values may be taken as indices of specificity of reaction, we are supplied with secondary evidence of the existence of specific reactions, for there are many reliable differences between

these S.D.'s. The P.E. of each of these measures is approximately 11% of its own value in the table classified by situations.

It may be observed again that the comparisons of specificity here made apply primarily to the experiment in hand. If the stimulating situations used are not typical samples of all situations, it is possible that a reaction which seems general here, for instance, is in fact common only to the particular situations chosen, and might therefore be rather specific in the long run. Naturally, this consideration in no way argues against the existence of specificity.

Since the figures show that there are differences in facial expression corresponding to the stimulus situation and to verbal reports of the subjects, it may be possible to eliminate the distinction Landis makes between the "natural" facial response and the "socialized" facial response. In order to reconcile his conclusions with the recognized ability of people to interpret posed expressions, Landis suggests that there is a socially conditioned language of emotional expressions which is brought into play when the subject intends to portray some feeling. Since these are different "natural" expressions, it may be that the intentional expressions have somewhat the same characteristics. Perhaps all expressions must be regarded as socially conditioned to some extent, or none of them so conditioned.

Summary

1. An examination of Landis' experiment on facial expression reveals an error in statistical procedure which invalidates his conclusions.

2. A correlational analysis of Landis' data was made to discover whether there were any facial reactions specific to stimulus situations or to subjects' reports.

3. The intercorrelations indicate that facial expressions in different circumstances have some common features and some which are peculiar to the situation at hand.

4. There may be a limited number of typical facial reaction patterns. But another possibility is suggested—that there is an indefinitely large number of facial expressions, each differing but slightly from its nearest kin.

5. Further analysis indicated which situations and reports had the most divergent expressions in this experiment, and which muscles had greater degrees of specificity in these situations.

Dr. Landis has very kindly read the foregoing paper and discussed it with the writer. Landis offers the following comments:

1. The criticism of his original statistical criterion is justified. He felt the criterion to be useful in the first place largely because it gave results agreeing with the appearance of the pictures to general inspection. (See above quotation from his article.)

2. The correlation technique of analysis used in the present paper is open to question on two counts: The head position should not be included in the computation, since it is not strictly facial expression, and not all items listed as muscles or muscle groups are independent. The upper and lower orbiculis oculi act reciprocally, and should be treated as one. Mentalis cannot be pursed without involving the lips. "Eyes up" is dependent upon the eye muscles previously listed. "Tension," "relaxation," and "smile" are complexes of a number of previously tabulated muscles.

3. Any statistical interpretation of the figures is difficult because of the manner in which the percentages of involvement were computed. Several photographs of each person's response to each situation were taken. At least one photograph of each person was included in the figures for each situation. But in cases where a person's later photographs, occasionally as many as five, showed a change from his first expression in a given situation, two or more photographs of that individual were included in the figures for that situation just as though the photographs represented different subjects. This is thought to load the percentages so as to make statistical analysis an uncertain procedure.

4. The chief conclusion of the present paper, that the nature of the stimulating situation plays a part in determining the nature of the facial response, and that there is also a correspondence between subject's report and facial response, is probably correct.[1]

In view of the objection to the correlation technique some further study was made. It is arbitrary, of course, whether facial expression is taken to include head position or not, but it is of interest to see what results may be obtained with that excluded. To investi-

[1]In this connection Landis calls attention to his conclusion Number 17: "It is tentatively suggested that the basis of distinction between emotions lies in the nature of the situation calling out the emotion and in the degree of general disturbance rather than in any specific differences in bodily or subjective reactions." To the writer, however, the meaning of this statement is precisely opposite to the conclusions of the present paper.

gate this question and the effect of Landis' other criticism a number of correlations were computed with a much abbreviated list of muscle groups. The upper and lower orbiculis oculi figures were averaged and used as one variable. Head positions, "tension," "relaxation," and "smile" were omitted. The mentalis has been omitted in the original correlations because of rarity of involvement. The abbreviated list is, therefore, frontalis, corrugators, orbiculis oculi, nasalis, quadrates, zygomaticus, risorius, "lips open," "lips pursed," triangularis, "eyes closed," "eyes wide." With the list a random sample of correlation coefficients was computed for comparison with those derived from the list of 20 variables. These are as shown in Table 8.

TABLE 8

CORRELATIONS BY SITUATION

Situations	Twenty variables	Twelve variables
1.2	.70	.59
9.12	.93	.75
2.5	.15	—.06
1.5	.13	—.13
4T.13	.42	.60
5.17	.55	.54
6.14	.89	.88
4T.14	.72	.55
13.17	.71	.79
Average	.58	.50

CORRELATIONS BY REPORT

Report	Twenty variables	Twelve variables
1.8	.43	.70
1.4	.86	.87
1.5	.94	.69
1.6	.80	.68
4.8	.49	.74
5.6	.34	.16
Average	.64	.65

With the shorter list some coefficients increase, others decrease, but the general position of the average appears not to be changed greatly. It still cannot be regarded as either 0 or 1.00.

Landis' comment number 3 is undoubtedly correct. In view of this method of computing percentages, it is necessary to specify that the conclusions apply to the average photograph used rather than to

the average person. To overcome this limitation and others which have appeared in the preceding discussion, it seems very desirable both to the writer and to Dr. Landis that the experiment be repeated with improved technique.

In discussing the statistical technique of the present data, Dr. J. Zubin has suggested that the Pearson square contingency (chi-square) would be applicable and involve fewer assumptions than the correlation procedure. (It would also yield less definite information.) Accordingly, a six-by-six table was constructed, using situations 1, 4T, 6, 10, 13, and 17 and the following muscles: frontalis, corrugators, nasalis, quadrates, risorius, and "lips open." Such a table should be sufficient to disprove a chance relation between variables. The percentages in Landis' table were taken as though they were numbers of cases in order to eliminate any effect of varying population totals in the columns. Chi-square was computed in the usual fashion and found to be 149.8. Pearson's table of the function gives *P,* the probability of chance occurrence for a value of chi-square, and the number of cells; but the table does not extend further than $X^2=70$, no. of cells=30. In such case the probability is .00003 that such a distribution would occur by chance. It is obvious without computation of the exact value that chance is not a reasonable explanation of the present data in which no. of cells=36 and $X^2=149$.

This method of treatment does not discriminate between the two situations, one resulting in a correlation of 0 and the other in a correlation of 1.00. But it does show that the data cannot be justifiably interpreted as chance deviations from either of these.

With these further tests the conclusions of the article appear to be sustained.

The writer desires to express appreciation for the excellent cooperation of Dr. Landis.

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LA SPÉCIFICITÉ DES EXPRESSIONS DE LA FIGURE

(Résumé)

La seule étude analytique des expressions de la figure dans des situations émotives est, parait-il, celle de Landis (1924) laquelle conclut qu'il n'existe aucune expression de la figure caractéristique d'une situation quelconque ou du rapport d'un sujet. L'examen du critière statistique de Landis montre qu'il est faux. On fait un analyse corrélative des données de Landis pour montrer si les muscles de la figure dans l'expérience (1) ont agi entièrement au hasard, (2) ont montré des déviations au hasard d'une seule forme de réaction de la figure, (3) ont montré nombre de formes différentes. Les résultats indiquent que la troisième alternative est l'interprétation correcte. On examine de plus les données pour découvrir les situations qui produisent des réponses semblables et celles qui produisent des réponses dissemblables, et les groupes musculaires qui montrent le plus de sélection dans leurs réponses. La spécificité de l'expression de la figure à certaines qualités de situation est indiquée par les données actuelles, mais ces données possèdent certains traits qui rendent désirable une autre expérience. On discute et teste quelques commentaires soumis par Landis sur les données précédentes; on applique le test "chi-carré" à quelques-unes des données. Les conclusions de l'article restent essentiellement non changées par ces considérations subséquentes.

DAVIS

DIE SPEZIFISCHE EIGENSCHAFT DER GESICHTSAUSDRÜCKE

(Referat)

Offenbar die einzige analytische Untersuchung von Gesichtsausdrücke bei Gefühlszuständen ist diejenige von Landis (1924), welche folgert, dass es kein Gesichtsausdruck gibt, der für irgend eine Situation oder irgend einen Bericht der Versuchsperson charakterisch ist. Eine Prüfung des statistischen Kriterions von Landis zeigt, dass dies unrichtig ist. Die Arbeit führt eine korrelative Analyse der Angaben von Landis durch, um zu zeigen, ob die Gesichtsmuskeln in dem betreffenden Experiment 1.) ganz aufs Geratewohl reagierten, 2.) zufällige Abweichungen von einem einzelnen (single) Prototypen der Gesichtsreaktionen aufwiesen, oder 3.) eine Anzahl verschiedener Prototypen zeigten. Die Ergebnisse erweisen die dritte Alternative als richtige Deutung. Die Angaben wurden ferner geprüft, um zu entdecken, welche Situationen ähnliche und welche unähnliche Reaktionen hervorrufen, und welche Muskelgruppen in diesen Reaktionen am meisten gewählt werden. Die spezifische Eigenschaft der Gesichtsausdrücke auf gewisse Situationsqualitäten wird durch die zur Hand liegenden Angaben erwiesen, aber gewisse Eigentümlichkeiten zeigen an, dass weitere Experimente wünschenswert sind. Man diskutiert und prüft eine von Landis unterbreitete Erklärungen über das oben Gesagte; der "Chi-square" Test wird auf einige der Angaben angewandt. Die Folgerungen der Arbeit werden durch die letztern Betrachtungen nicht wesentlich verändert.

DAVIS

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ANTHROPOLOGY AND THE ABNORMAL*

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Modern social anthropology has become more and more a study the varieties and common elements of cultural environment and e consequences of these in human behavior. For such a study of verse social orders primitive peoples fortunately provide a labora- ry not yet entirely vitiated by the spread of a standardized world- de civilization. Dyaks and Hopis, Fijians and Yakuts are signi- ant for psychological and sociological study because only among ese simpler peoples has there been sufficient isolation to give op- rtunity for the development of localized social forms. In the gher cultures the standardization of custom and belief over a uple of continents has given a false sense of the inevitability of e particular forms that have gained currency, and we need to turn a wider survey in order to check the conclusions we hastily base on this near-universality of familiar customs. Most of the simpler ltures did not gain the wide currency of the one which, out of our perience, we identify with human nature, but this was for various torical reasons, and certainly not for any that gives us as its rriers a monopoly of social good or of social sanity. Modern ilization, from this point of view, becomes not a necessary pinnacle human achievement but one entry in a long series of possible ad- tments.

These adjustments, whether they are in mannerisms like the ways showing anger, or joy, or grief in any society, or in major human ves like those of sex, prove to be far more variable than experience any one culture would suggest. In certain fields, such as that of igion or of formal marriage arrangements, these wide limits of iability are well known and can be fairly described. In others s not yet possible to give a generalized account, but that does not

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absolve us of the task of indicating the significance of the work that has been done and of the problems that have arisen.

One of these problems relates to the customary modern normal-abnormal categories and our conclusions regarding them. In how far are such categories culturally determined, or in how far can we with assurance regard them as absolute? In how far can we regard inability to function socially as diagnostic of abnormality, or in how far is it necessary to regard this as a function of the culture?

As a matter of fact, one of the most striking facts that emerge from a study of widely varying cultures is the ease with which our abnormals function in other cultures. It does not matter what kind of "abnormality" we choose for illustration, those which indicate extreme instability, or those which are more in the nature of character traits like sadism or delusions of grandeur or of persecution, there are well-described cultures in which these abnormals function at ease and with honor, and apparently without danger or difficulty to the society.

The most notorious of these is trance and catalepsy. Even a very mild mystic is aberrant in our culture. But most peoples have regarded even extreme psychic manifestations not only as normal and desirable, but even as characteristic of highly valued and gifted individuals. This was true even in our own cultural background in that period when Catholicism made the ecstatic experience the mark of sainthood. It is hard for us, born and brought up in a culture that makes no use of the experience, to realize how important a rôle it may play and how many individuals are capable of it, once it has been given an honorable place in any society.

Some of the Indian tribes of California accorded prestige principally to those who passed through certain trance experiences. Not all of these tribes believed that it was exclusively women who were so blessed, but among the Shasta (10) this was the convention. Their shamans were women, and they were accorded the greatest prestige in the community. They were chosen because of their constitutional liability to trance and allied manifestations. One day the woman who was so destined, while she was about her usual work, would fall suddenly to the ground. She had heard a voice speaking to her in tones of the greatest intensity. Turning, she had seen a man with drawn bow and arrow. He commanded her to sing on pain of being shot through the heart by his arrow, but under

the stress of the experience she fell senseless. Her family gathered. She was lying rigid, hardly breathing. They knew that for some time she had had dreams of a special character which indicated a shamanistic calling, dreams of escaping grizzly bears, falling off cliffs or trees, or of being surrounded by swarms of yellow jackets. The community knew therefore what to expect. After a few hours the woman began to moan gently and to roll about upon the ground, trembling violently. She was supposed to be repeating the song which she had been told to sing and which during the trance had been taught her by the spirit. As she revived her moaning became more and more clearly the spirit's song until at last she called out the name of the spirit itself, and immediately blood oozed from her mouth.

When the woman had come to herself after the first encounter with her spirit she danced that night her first initiatory shamanistic dance, holding herself by a rope that was swung from the ceiling. For three nights she danced, and on the third night she had to receive in her body her power from her spirit. She was dancing, and as she felt the approach of the moment she called out, "He will shoot me, he will shoot me." Her friends stood close, for when she reeled in a kind of cataleptic seizure, they had to seize her before she fell or she would die. From this time on she had in her body a visible materialization of her spirit's power, an icicle-like object which in her dances thereafter she would exhibit, producing it from one part of her body and returning it to another part. From this time on she continued to validate her supernatural power by further cataleptic demonstrations, and she was called upon in great emergencies of life and death, for curing and for divination and for counsel. She became in other words by this procedure a woman of great power and importance.[1]

It is clear that, so far from regarding cataleptic seizures as blots upon the family escutcheon and as evidences of dreaded disease, cultural approval had seized upon them and made of them the path-

[1]In all cultures behavior which is socially rewarded attracts persons who are attracted by the possibility of leadership, and such individuals may simulate the required behavior. This is as true when society rewards prodigality as when it rewards catalepsy. For the present argument the amount of shamming is not considered though it is of obvious importance. It is a matter which cultures standardize quite as much as they standardize the type of rewarded behavior.

way to authority over one's fellows. They were the outstanding characteristic of the most respected social type, the type which functioned with most honor and reward in the community. It was precisely the cataleptic individuals who in this culture were singled out for authority and leadership.

The availability of "abnormal" types in the social structure, provided they are types that are culturally selected by that group, is illustrated from every part of the world. The shamans of Siberia dominate their communities. According to the ideas of these peoples, they are individuals who by submission to the will of the spirits have been cured of a grievous illness—the onset of the seizures—and have acquired by this means great supernatural power and incomparable vigor and health. Some, during the period of the call, are violently insane for several years, others irresponsible to the point where they have to be watched constantly lest they wander off in the snow and freeze to death, others ill and emaciated to the point of death, sometimes with bloody sweat. It is the shamanistic practice which constitutes their cure, and the extreme physical exertion of a Siberian seance leaves them, they claim, rested and able to enter immediately upon a similar performance. Cataleptic sezures are regarded as an essential part of any shamanistic performance (8).

A good description of the neurotic condition of the shaman and the attention given him by his society is an old one by Canon Callaway (6, pp. 259 ff.) recorded in the words of an old Zulu of South Africa:

> "The condition of a man who is about to become a diviner is this; at first he is apparently robust, but in the process of time he begins to be delicate, not having any real disease, but being delicate. He habitually avoids certain kinds of food, choosing what he likes, and he does not eat much of that; he is continually complaining of pains in different parts of his body. And he tells them that he has dreamt that he was carried away by a river. He dreams of many things, and his body is muddied (as a river) and he becomes a house of dreams. He dreams constantly of many things, and on awaking tells his friends, 'My body is muddied today; I dreamt many men were killing me, and I escaped I know not how. On waking one part of my body felt different from other parts; it was no longer alike all over.' At last that man is very ill, and they go to the diviners to enquire.

"The diviners do not at once see that he is about to have a soft head (that is, the sensitivity associated with shamanism). It is difficult for them to see the truth; they continually talk nonsense and make false statements, until all the man's cattle are devoured at their command, they saying that the spirit of his people demands cattle, that it may eat food. At length all the man's property is expended, he still being ill; and they no longer know what to do, for he has no more cattle, and his friends help him in such things as he needs.

"At length a diviner comes and says that all the others are wrong. He says, 'He is possessed by the spirits. There is nothing else. They move in him, being divided into two parties; some say, "No, we do not wish our child injured. We do not wish it." It is for that reason he does not get well. If you bar the way against the spirits, you will be killing him. For he will not be a diviner; neither will he ever be a man again.'

"So the man may be ill two years without getting better; perhaps even longer than that. He is confined to his house. This continues till his hair falls off. And his body is dry and scurfy; he does not like to anoint himself. He shows that he is about to be a diviner by yawning again and again, and by sneezing continually. It is apparent also from his being very fond of snuff; not allowing any long time to pass without taking some. And people begin to see that he has had what is good given to him.

"After that he is ill; he has convulsions, and when water has been poured on him they then cease for a time. He habitually sheds tears, at first slight, then at last he weeps aloud and when the people are asleep he is heard making a noise and wakes the people by his singing; he has composed a song, and the men and women awake and go to sing in concert with him. All the people of the village are troubled by want of sleep; for a man who is becoming a diviner causes great trouble, for he does not sleep, but works constantly with his brain; his sleep is merely by snatches, and he wakes up singing many songs; and people who are near quit their villages by night when they hear him singing aloud and go to sing in concert. Perhaps he sings till morning, no one having slept. And then he leaps about the house like a frog; and the house becomes too small for him, and he goes out leaping and singing, and shaking like a reed in the water, and dripping with perspiration.

> "In this state of things they daily expect his death; he is now but skin and bones, and they think that tomorrow's sun will not leave him alive. At this time many cattle are eaten, for the people encourage his becoming a diviner. At length (in a dream) an ancient ancestral spirit is pointed out to him. This spirit says to him, 'Go to So-and-so and he will churn for you an emetic (the medicine the drinking of which is a part of shamanistic initiation) that you may be a diviner altogether.' Then he is quiet a few days, having gone to the diviner to have the medicine churned for him; and he comes back quite another man, being now cleansed and a diviner indeed."

Thereafter for life when he achieves possession, he fortells events, and finds lost articles.

It is clear that culture may value and make socially available even highly unstable human types. If it chooses to treat their peculiarities as the most valued variants of human behavior, the individuals in question will rise to the occasion and perform their social rôles without reference to our usual ideas of the types who can make social adjustments and those who cannot.

Cataleptic and trance phenomena are, of course, only one illustration of the fact that those whom we regard as abnormals may function adequately in other cultures. Many of our culturally discarded traits are selected for elaboration in different societies. Homosexuality is an excellent example, for in this case our attention is not constantly diverted, as in the consideration of trance, to the interruption of routine activity which it implies. Homosexuality poses the problem very simply. A tendency toward this trait in our culture exposes an individual to all the conflicts to which all aberrants are always exposed, and we tend to identify the consequences of this conflict with homosexuality. But these consequences are obviously local and cultural. Homosexuals in many societies are not incompetent, but they may be such if the culture asks adjustments of them that would strain any man's vitality. Wherever homosexuality has been given an honorable place in any society, those to whom it is congenial have filled adequately the honorable rôles society assigns to them. Plato's *Republic* is, of course, the most convincing statement of such a reading of homosexuality. It is presented as one of the major means to the good life, and it was generally so regarded in Greece at that time.

The cultural attitude toward homosexuals has not always been on such a high ethical plane, but it has been very varied. Among many American Indian tribes there exists the institution of the berdache (12, 15), as the French called them. These men-women were men who at puberty or thereafter took the dress and the occupations of women. Sometimes they married other men and lived with them. Sometimes they were men with no inversion, persons of weak sexual endowment who chose this rôle to avoid the jeers of the women. The berdaches were never regarded as of first-rate supernatural power, as similar men-women were in Siberia, but rather as leaders in women's occupations, good healers in certain diseases, or, among certain tribes, as the genial organizers of social affairs. In any case, they were socially placed. They were not left exposed to the conflicts that visit the deviant who is excluded from participation in the recognized patterns of his society.

The most spectacular illustrations of the extent to which normality may be culturally defined are those cultures where an abnormality of our culture is the cornerstone of their social structure. It is not possible to do justice to these possibilities in a short discussion. A recent study of an island of northwest Melanesia by Fortune (11) describes a society built upon traits which we regard as beyond the border of paranoia. In this tribe the exogamic groups look upon each other as prime manipulators of black magic, so that one marries always into an enemy group which remains for life one's deadly and unappeasable foes. They look upon a good garden crop as a confession of theft, for everyone is engaged in making magic to induce into his garden the productiveness of his neighbors'; therefore no secrecy in the island is so rigidly insisted upon as the secrecy of a man's harvesting of his yams. Their polite phrase at the acceptance of a gift is, "And if you now poison me, how shall I repay you this present?" Their preoccupation with poisoning is constant; no woman ever leaves her cooking pot for a moment untended. Even the great affinal economic exchanges that are characteristic of this Melanesian culture area are quite altered in Dobu since they are incompatible with this fear and distrust that pervades the culture. They go farther and people the whole world outside their own quarters with such malignant spirits that all-night feasts and ceremonials simply do not occur here. They have even rigorous religiously enforced customs that forbid the sharing of seed even in one family group. Any-

one else's food is deadly poison to you, so that communality of stores is out of the question. For some months before harvest the whole society is on the verge of starvation, but if one falls to the temptation and eats up one's seed yams, one is an outcast and a beachcomber for life. There is no coming back. It involves, as a matter of course, divorce and the breaking of all social ties.

Now in this society where no one may work with another and no one may share with another, Fortune describes the individual who was regarded by all his fellows as crazy. He was not one of those who periodically ran amok and, beside himself and frothing at the mouth, fell with a knife upon anyone he could reach. Such behavior they did not regard as putting anyone outside the pale. They did not even put the individuals who were known to be liable to these attacks under any kind of control. They merely fled when they saw the attack coming on and kept out of the way. "He would be all right tomorrow." But there was one man of sunny, kindly disposition who liked work and liked to be helpful. The compulsion was too strong for him to repress it in favor of the opposite tendencies of his culture. Men and women never spoke of him without laughing; he was silly and simple and definitely crazy. Nevertheless, to the ethnologist used to a culture that has, in Christianity, made his type the model of all virtue, he seemed a pleasant fellow.

An even more extreme example, because it is of a culture that has built itself upon a more complex abnormality, is that of the North Pacific Coast of North America. The civilization of the Kwakiutl (1-5), at the time when it was first recorded in the last decades of the nineteenth century, was one of the most vigorous in North America. It was built up on an ample economic supply of goods, the fish which furnished their food staple being practically inexhaustible and obtainable with comparatively small labor, and the wood which furnished the material for their houses, their furnishings, and their arts being, with however much labor, always procurable. They lived in coastal villages that compared favorably in size with those of any other American Indians and they kept up constant communication by means of sea-going dug-out canoes.

It was one of the most vigorous and zestful of the aboriginal cultures of North America, with complex crafts and ceremonials, and elaborate and striking arts. It certainly had none of the earmarks of a sick civilization. The tribes of the Northwest Coast had wealth,

and exactly in our terms. That is, they had not only a surplus of economic goods, but they made a game of the manipulation of wealth. It was by no means a mere direct transcription of economic needs and the filling of those needs. It involved the idea of capital, of interest, and of conspicuous waste. It was a game with all the binding rules of a game, and a person entered it as a child. His father distributed wealth for him, according to his ability, at a small feast or potlatch, and each gift the receiver was obliged to accept and to return after a short interval with interest that ran to about 100 per cent a year. By the time the child was grown, therefore, he was well launched, a larger potlatch had been given for him on various occasions of exploit or initiation, and he had wealth either out at usury or in his own possession. Nothing in the civilization could be enjoyed without validating it by the distribution of this wealth. Everything that was valued, names and songs as well as material objects, were passed down in family lines, but they were always publicly assumed with accompanying sufficient distributions of property. It was the game of validating and exercising all the privileges one could accumulate from one's various forbears, or by gift, or by marriage, that made the chief interest of the culture. Everyone in his degree took part in it, but many, of course, mainly as spectators. In its highest form it was played out between rival chiefs representing not only themselves and their family lines but their communities, and the object of the contest was to glorify oneself and to humiliate one's opponent. On this level of greatness the property involved was no longer represented by blankets, so many thousand of them to a potlatch, but by higher units of value. These higher units were like our bank notes. They were incised copper tablets, each of them named, and having a value that depended upon their illustrious history. This was as high as ten thousand blankets, and to possess one of them, still more to enhance its value at a great potlatch, was one of the greatest glories within the compass of the chiefs of the Northwest Coast.

The details of this manipulation of wealth are in many ways a parody on our own economic arrangements, but it is with the motivations that were recognized in this contest that we are concerned in this discussion. The drives were those which in our own culture we should call megalomaniac. There was an uncensored self-glorification and ridicule of the opponent that it is hard to equal in other

cultures outside of the monologues of the abnormal. Any of the songs and speeches of their chiefs at a potlatch illustrate the usual tenor:

. . . . . .

Wa, out of the way. Wa, out of the way. Turn your faces that I may give way to my anger by striking my fellow chiefs.

Wa, great potlatch, greatest potlatch.[2] The little ones[3] only pretend, the little stubborn ones, they only sell one copper again and again and give it away to the little chiefs of the tribe.
Ah, do not ask in vain for mercy. Ah, do not ask in vain for mercy and raise your hands, you with lolling tongues! I shall break,[4] I shall let disappear the great copper that has the name Kentsegum, the property of the great foolish one, the great extravagant one, the great surpassing one, the one farthest ahead, the great Cannibal Dancer among the chiefs.[5]

I am the great chief who makes people ashamed.
I am the great chief who makes people ashamed.
Our chief brings shame to the faces.
Our chief brings jealousy to the faces.
Our chief makes people cover their faces by what he is continually doing in this world, from the beginning to the end of the year,
Giving again and again oil feasts to the tribes.

I am the great chief who vanquishes.
I am the great chief who vanquishes.
Only at those who continue running round and round in this world, working hard, losing their tails,[6] I sneer, at the chiefs below the true chief.[7]
Have mercy on them![8] Put oil on their dry heads with brittle hair, those who do not comb their hair!

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[2]The feast he is now engaged in giving.
[3]His opponents.
[4]To break a copper, showing in this way how far one rose above even the most superlatively valuable things, was the final mark of greatness.
[5]Himself.
[6]As salmon do.
[7]Himself.
[8]Irony, of course.

I sneer at the chiefs below the true, real chief. I am the great chief who makes people ashamed.

. . . . . .

I am the only great tree, I the chief.
I am the only great tree, I the chief.
You are my subordinates, tribes.
You sit in the middle of the rear of the house, tribes.
Bring me your counter of property, tribes, that he may in vain try to count what is going to be given away by the great copper-maker, the chief.
Oh, I laugh at them, I sneer at them who empty boxes[9] in their houses, their potlatch houses, their inviting houses that are full only of hunger. They follow along after me like young sawbill ducks. I am the only great tree, I the chief.

. . . . . .

I have quoted a number of these hymns of self-glorification because by an association which psychiatrists will recognize as fundamental these delusions of grandeur were essential in the paranoid view of life which was so strikingly developed in this culture. All of existence was seen in terms of insult.[9a] Not only derogatory acts performed by a neighbor or an enemy, but all untoward events, like a cut when one's axe slipped, or a ducking when one's canoe overturned, were insults. All alike threatened first and foremost one's ego security, and the first thought one was allowed was how to get even, how to wipe out the insult. Grief was little institutionalized, but sulking took its place. Until he had resolved upon a course of action by which to save his face after any misfortune, whether it was the slipping of a wedge in felling a tree, or the death of a favorite child, an Indian of the Northwest Coast retired to his pallet with his face to the wall and neither ate nor spoke. He rose from it to follow out some course which according to the traditional rules should reinstate him in his own eyes and those of the community: to distribute property enough to wipe out the stain, or to go head-hunting in order that somebody else should be made to mourn. His activities in neither case were specific responses to the bereavement

[9]Of treasure.

[9a]Insult is used here in reference to the intense susceptibility to shame that is conspicuous in this culture. All possible contingencies were interpreted as rivalry situations, and the gamut of emotions swung between triumph and shame.

he had just passed through, but were elaborately directed toward getting even. If he had not the money to distribute and did not succeed in killing someone to humiliate another, he might take his own life. He had staked everything, in his view of life, upon a certain picture of the self, and, when the bubble of his self-esteem was pricked, he had no interest, no occupation to fall back on, and the collapse of his inflated ego left him prostrate.

Every contingency of life was dealt with in these two traditional ways. To them the two were equivalent. Whether one fought with weapons or "fought with property," as they say, the same idea was at the bottom of both. In the olden times, they say, they fought with spears, but now they fight with property. One overcomes one's opponents in equivalent fashion in both, matching forces and seeing that one comes out ahead, and one can thumb one's nose at the vanquished rather more satisfactorily at a potlatch than on a battle field. Every occasion in life was noticed, not in its own terms, as a stage in the sex life of the individual or as a climax of joy or of grief, but as furthering this drama of consolidating one's own prestige and bringing shame to one's guests. Whether it was the occasion of the birth of a child, or a daughter's adolescence, or of the marriage of one's son, they were all equivalent raw material for the culture to use for this one traditionally selected end. They were all to raise one's own personal status and to entrench oneself by the humiliation of one's fellows. A girl's adolescence among the Nootka (16) was an event for which her father gathered property from the time she was first able to run about. When she was adolescent he would demonstrate his greatness by an unheard of distribution of these goods, and put down all his rivals. It was not as a fact of the girl's sex life that it figured in their culture, but as the occasion for a major move in the great game of vindicating one's own greatness and humiliating one's associates.

In their behavior at great bereavements this set of the culture comes out most strongly. Among the Kwakiutl it did not matter whether a relative had died in bed of disease, or by the hand of an enemy, in either case death was an affront to be wiped out by the death of another person. The fact that one had been caused to mourn was proof that one had been put upon. A chief's sister and her daughter had gone up to Victoria, and either because they drank bad whiskey or because their boat capsized they never came back.

The chief called together his warriors. "Now I ask you, tribes, who shall wail? Shall I do it or shall another?" The spokesman answered, of course, "Not you, Chief. Let some other of the tribes." Immediately they set up the war pole to announce their intention of wiping out the injury, and gathered a war party. They set out, and found seven men and two children asleep and killed them. "Then they felt good when they arrived at Sebaa in the evening."

The point which is of interest to us is that in our society those who on that occasion would feel good when they arrived at Sebaa that evening would be the definitely abnormal. There would be some, even in our society, but it is not a recognized and approved mood under the circumstances. On the Northwest Coast those are favored and fortunate to whom that mood under those circumstances is congenial, and those to whom it is repugnant are unlucky. This latter minority can register in their own culture only by doing violence to their congenial responses and acquiring others that are difficult for them. The person, for instance, who, like a Plains Indian whose wife has been taken from him, is too proud to fight, can deal with the Northwest Coast civilization only by ignoring its strongest bents. If he cannot achieve it, he is the deviant in that culture, their instance of abnormality.

This head-hunting that takes place on the Northwest Coast after a death is no matter of blood revenge or of organized vengeance. There is no effort to tie up the subsequent killing with any responsibility on the part of the victim for the death of the person who is being mourned. A chief whose son has died goes visiting whereever his fancy dictates, and he says to his host, "My prince has died today, and you go with him." Then he kills him. In this, according to their interpretation, he acts nobly because he has not been downed. He has thrust back in return. The whole procedure is meaningless without the fundamental paranoid reading of bereavement. Death, like all the other untoward accidents of existence, confounds man's pride and can only be handled in the category of insults.

Behavior honored upon the Northwest Coast is one which is recognized as abnormal in our civilization, and yet it is sufficiently close to the attitudes of our own culture to be intelligible to us and to have a definite vocabulary with which we may discuss it. The megalomaniac paranoid trend is a definite danger in our society. It

is encouraged by some of our major preoccupations, and it confronts us with a choice of two possible attitudes. One is to brand it as abnormal and reprehensible, and is the attitude we have chosen in our civilization. The other is to make it an essential attribute of ideal man, and this is the solution in the culture of the Northwest Coast.

These illustrations, which it has been possible to indicate only in the briefest manner, force upon us the fact that normality is culturally defined. An adult shaped to the drives and standards of either of these cultures, if he were transported into our civilization, would fall into our categories of abnormality. He would be faced with the psychic dilemmas of the socially unavailable. In his own culture, however, he is the pillar of society, the end result of socially inculcated mores, and the problem of personal instability in his case simply does not arise.

No one civilization can possibly utilize in its mores the whole potential range of human behavior. Just as there are great numbers of possible phonetic articulations, and the possibility of language depends on a selection and standardization of a few of these in order that speech communication may be possible at all, so the possibility of organized behavior of every sort, from the fashions of local dress and houses to the dicta of a people's ethics and religion, depends upon a similar selection among the possible behavior traits. In the field of recognized economic obligations or sex tabus this selection is as nonrational and subconscious a process as it is in the field of phonetics. It is a process which goes on in the group for long periods of time and is historically conditioned by innumerable accidents of isolation or of contact of peoples. In any comprehensive study of psychology, the selection that different cultures have made in the course of history within the great circumference of potential behavior is of great significance.

Every society,[10] beginning with some slight inclination in one direction or another, carries its preference farther and farther, integrating itself more and more completely upon its chosen basis, and discarding those types of behavior that are uncongenial. Most of those organizations of personality that seem to us most incontrovertibly abnormal

[10]This phrasing of the process is deliberately animistic. It is used with no reference to a group mind or a superorganic, but in the same sense in which it is customary to say, "Every art has its own canons."

have been used by different civilizations in the very foundations of their institutional life. Conversely the most valued traits of our normal individuals have been looked on in differently organized cultures as aberrant. Normality, in short, within a very wide range, is culturally defined. It is primarily a term for the socially elaborated segment of human behavior in any culture; and abnormality, a term for the segment that that particular civilization does not use. The very eyes with which we see the problem are conditioned by the long traditional habits of our own society.

It is a point that has been made more often in relation to ethics than in relation to psychiatry. We do not any longer make the mistake of deriving the morality of our own locality and decade directly from the inevitable constitution of human nature. We do not elevate it to the dignity of a first principle. We recognize that morality differs in every society, and is a convenient term for socially approved habits. Mankind has always preferred to say, "It is a morally good," rather than "It is habitual," and the fact of this preference is matter enough for a critical science of ethics. But historically the two phrases are synonymous.

The concept of the normal is properly a variant of the concept of the good. It is that which society has approved. A normal action is one which falls well within the limits of expected behavior for a particular society. Its variability among different peoples is essentially a function of the variability of the behavior patterns that different societies have created for themselves, and can never be wholly divorced from a consideration of culturally institutionalized types of behavior.

Each culture is a more or less elaborate working-out of the potentialities of the segment it has chosen. In so far as a civilization is well integrated and consistent within itself, it will tend to carry farther and farther, according to its nature, its initial impulse toward a particular type of action, and from the point of view of any other culture those elaborations will include more and more extreme and aberrant traits.

Each of these traits, in proportion as it reinforces the chosen behavior patterns of that culture, is for that culture normal. Those individuals to whom it is congenial either congenitally, or as the result of childhood sets, are accorded prestige in that culture, and are not visited with the social contempt or disapproval which their

traits would call down upon them in a society that was differently organized. On the other hand, those individuals whose characteristics are not congenial to the selected type of human behavior in that community are the deviants, no matter how valued their personality traits may be in a contrasted civilization.

The Dobuan who is not easily susceptible to fear of treachery, who enjoys work and likes to be helpful, is their neurotic and regarded as silly. On the Northwest Coast the person who finds it difficult to read life in terms of an insult contest will be the person upon whom fall all the difficulties of the culturally unprovided for. The person who does not find it easy to humiliate a neighbor, nor to see humiliation in his own experience, who is genial and loving, may, of course, find some unstandardized way of achieving satisfactions in his society, but not in the major patterned responses that his culture requires of him. If he is born to play an important rôle in a family with many hereditary privileges, he can succeed only by doing violence to his whole personality. If he does not succeed, he has betrayed his culture; that is, he is abnormal.

I have spoken of individuals as having sets toward certain types of behavior, and of these sets as running sometimes counter to the types of behavior which are institutionalized in the culture to which they belong. From all that we know of contrasting cultures it seems clear that differences of temperament occur in every society. The matter has never been made the subject of investigation, but from the available material it would appear that these temperament types are very likely of universal recurrence. That is, there is an ascertainable range of human behavior that is found wherever a sufficiently large series of individuals is observed. But the proportion in which behavior types stand to one another in different societies is not universal. The vast majority of the individuals in any group are shaped to the fashion of that culture. In other words, most individuals are plastic to the moulding force of the society into which they are born. In a society that values trance, as in India, they will have supernormal experience. In a society that institutionalizes homosexuality, they will be homosexual. In a society that sets the gathering of possessions as the chief human objective, they will amass property. The deviants, whatever the type of behavior the culture has institutionalized, will remain few in number, and there seems no more difficulty in moulding the vast malleable majority to the

"normality" of what we consider an aberrant trait, such as delusions of reference, than to the normality of such accepted behavior patterns as acquisitiveness. The small proportion of the number of the deviants in any culture is not a function of the sure instinct with which that society has built itself upon the fundamental sanities, but of the universal fact that, happily, the majority of mankind quite readily take any shape that is presented to them.

The relativity of normality is not an academic issue. In the first place, it suggests that the apparent weakness of the aberrant is most often and in great measure illusory. It springs not from the fact that he is lacking in necessary vigor, but that he is an individual upon whom that culture has put more than the usual strain. His inability to adapt himself to society is a reflection of the fact that that adaptation involves a conflict in him that it does not in the so-called normal.

Therapeutically, it suggests that the inculcation of tolerance and appreciation in any society toward its less usual types is fundamentally important in successful mental hygiene. The complement of this tolerance, on the patients' side, is an education in self-reliance and honesty with himself. If he can be brought to realize that what has thrust him into his misery is despair at his lack of social backing he may be able to achieve a more independent and less tortured attitude and lay the foundation for an adequately functioning mode of existence.

There is a further corollary. From the point of view of absolute categories of abnormal psychology, we must expect in any culture to find a large proportion of the most extreme abnormal types among those who from the local point of view are farthest from belonging to this category. The culture, according to its major preoccupations, will increase and intensify hysterical, epileptic, or paranoid symptoms, at the same time relying socially in a greater and greater degree upon these very individuals. Western civilization allows and culturally honors gratifications of the ego which according to any absolute category would be regarded as abnormal. The portrayal of unbridled and arrogant egoists as family men, as officers of the law, and in business has been a favorite topic of novelists, and they are familiar in every community. Such individuals are probably mentally warped to a greater degree than many inmates of our institutions who are nevertheless socially unavailable. They are ex-

treme types of those personality configurations which our civilization fosters.

This consideration throws into great prominence the confusion that follows on the one hand, the use of social inadequacy as a criterion of abnormality and, on the other, of definite fixed symptoms. The confusion is present in practically all discussions of abnormal psychology, and it can be clarified chiefly by adequate consideration of the character of the culture, not of the constitution of the abnormal individual. Nevertheless, the bearing of social security upon the total situation of the abnormal cannot be exaggerated, and the study of comparative psychiatry will be fundamentally concerned with this aspect of the matter.

It is clear that statistical methods of defining normality, so long as they are based on studies in a selected civilization, only involve us, unless they are checked against the cultural configuration, in deeper and deeper provincialism. The recent tendency in abnormal psychology to take the laboratory mode as normal and to define abnormalities as they depart from this average has value in so far as it indicates that the aberrants in any culture are those individuals who are liable to serious disturbances because their habits are culturally unsupported. On the other hand, it overlooks the fact that every culture besides its abnormals of conflict has presumably its abnormals of extreme fulfillment of the cultural type. From the point of view of a universally valid abnormal psychology the extreme types of abnormality would probably be found in this very group—a group which in every study based upon one culture goes undescribed except in its end institutionalized forms.

The relativity of normality is important in what may some day come to be a true social engineering. Our picture of our own civilization is no longer in this generation in terms of a changeless and divinely derived set of categorical imperatives. We must face the problems our changed perspective has put upon us. In this matter of mental ailments, we must face the fact that even our normality is man-made, and is of our own seeking. Just as we have been handicapped in dealing with ethical problems so long as we held to an absolute definition of morality, so too in dealing with the problems of abnormality we are handicapped so long as we identify our local normalities with the universal sanities. I have taken illustrations from different cultures, because the conclusions are most inescapable

from the contrasts as they are presented in unlike social groups. But the major problem is not a consequence of the variability of the normal from culture to culture, but its variability from era to era. This variability in time we cannot escape if we would, and it is not beyond the bounds of possibility that we may be able to face this inevitable change with full understanding and deal with it rationally (9). No society has yet achieved self-conscious and critical analysis of its own normalities and attempted rationally to deal with its own social process of creating new normalities within its next generation. But the fact that it is unachieved is not therefore proof of its impossibility. It is a faint indication of how momentous it could be in human society.

There is another major factor in the cultural conditioning of abnormality. From the material that is available at the present time it seems a lesser factor than the one we have discussed. Nevertheless, disregard of its importance has led to many misconceptions. The particular forms of behavior to which unstable individuals of any group are liable are many of them matters of cultural patterning like any other behavior. It is for this obvious reason that the epidemic disorders of one continent or era are often rare or unreported from other parts of the world or other periods of history.

The baldest evidences of cultural patterning in the behavior of unstable individuals is in trance phenomena. The use to which such proclivities are put, the form their manifestations take, the things that are seen and felt in trance, are all culturally controlled. The tranced individual may come back with communications from the dead describing the minutiae of life in the hereafter, or he may visit the world of the unborn, or get information about lost objects in the camp, or experience cosmic unity, or acquire a life-long guardian spirit, or get information about coming events. Even in trance the individual holds strictly to the rules and expectations of his culture, and his experience is as locally patterned as a marriage rite or an economic exchange.

The conformity of trance experience to the expectations of waking life is well recognized. Now that we are no longer confused by the attempt to ascribe supernormal validity to the one or the other, and realize how trance experience bodies forth the preoccupations of the experiencing individual, the cultural patterning in ecstasy has become an accepted tenet.

But the matter does not end here. It is not only what is seen in trance experience that has clear-cut geographical and temporal distribution. It is equally true of forms of behavior which are affected by certain unstable individuals in any group. It is one of the prime difficulties in the use of such unprecise and casual information as we possess about the behavior of the unstable in different cultures, that the material does not correspond to data from our own society. It has even been thought that such definite types of instability as Arctic hysteria (14) and the Malay running-amok were racial diseases. But we know at least, in spite of the lack of good psychiatric accounts, that these phenomena do not coincide with racial distributions. Moreover, the same problem is quite as striking in cases where there is no possibility of a racial correlation. Running amok has been described as alike in symptoms and alike in the treatment accorded it by the rest of the group from such different parts of the world as Melanesia (11, pp. 54-55) and Tierra del Fuego (7).

The racial explanation is also ruled out of court in those instances of epidemic mania which are characteristic of our own cultural background. The dancing mania (13) that filled the streets of Europe with compulsively dancing men, women, and children in mediaeval times is recognized as an extreme instance of suggestibility in our own racial group.

These behaviors are capable of controlled elaboration that is often carried to great lengths. Unstable individuals in one culture achieve characteristic forms that may be excessively rare or absent in another, and this is very marked where social value has been attached to one form or another. Thus when some form of borderline behavior has been associated in any society with the shaman and he is a person of authority and influence, it is this particular indicated seizure to which he will be liable at every demonstration. Among the Shasta of California, as we have seen, and among many other tribes in various parts of the world, some form of cataleptic seizure is the passport to shamanism and must constantly accompany its practice. In other regions it is automatic vision or audition. In other societies behavior is perhaps closest to what we cover by the term hystero-epilepsy. In Siberia all the familiar characteristics of our spiritualistic seances are required for every performance of the shaman. In all these cases the particular experience that is thus socially chosen receives considerable elaboration and is usually pat-

terned in detail according to local standards. That is, each culture, though it chooses quite narrowly in the great field of borderline experiences, without difficulty imposes its selected type upon certain of its individuals. The particular behavior of an unstable individual in these instances is not the single and inevitable mode in which his abnormality could express itself. He has taken up a traditionally conditioned pattern of behavior in this as in any other field. Conversely, in every society, our own included, there are forms of instability that are out of fashion. They are not at the present time at least being presented for imitation to the enormously suggestible individuals who constitute in any society a considerable group of the abnormals. It seems clear that this is no matter of the nature of sanity, or even of a biological, inherited tendency in a local group, but quite simply an affair of social patterning.

The problem of understanding abnormal human behavior in any absolute sense independent of cultural factors is still far in the future. The categories of borderline behavior which we derive from the study of the neuroses and psychoses of our civilization are categories of prevailing local types of instability. They give much information about the stresses and strains of Western civilization, but no final picture of inevitable human behavior. Any conclusions about such behavior must await the collection by trained observers of psychiatric data from other cultures. Since no adequate work of the kind has been done at the present time, it is impossible to say what core of definition of abnormality may be found valid from the comparative material. It is as it is in ethics: all our local conventions of moral behavior and of immoral are without absolute validity, and yet it is quite possible that a modicum of what is considered right and what wrong could be disentangled that is shared by the whole human race. When data are available in psychiatry, this minimum definition of abnormal human tendencies will be probably quite unlike our culturally conditioned, highly elaborated psychoses such as those that are described, for instance, under the terms of schizophrenia and manic-depressive.

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LA CULTURE ET LES ANORMAUX

(Résumé)

L'anormalité, en tant qu'elle se rapporte aux organisations variantes de la personnalité, est définie au point de vue de la culture. Elle a bien entendu ses déterminants organiques, mais les symptômes et le fonctionnement social des anormaux sont relatives à la culture. La plupart de ces organisations de la personnalité lesquelles nous semblent anormales ont été employées par de diverses civilisations dans les fondements de leur vie prescrite, et les individus qui représentent ces types fonctionnent en tels cas avec aise et avec honneur dans la société. Contrairement les traits estimés de nos individus normaux ont été considérés anormaux par des sociétés différemment organisées, et en tels cas les personnes qui possèdent ces traits ne sont pas soutenues par la société et ne fonctionnent pas d'une façon satisfaisante.

On peut appuyer sur deux aspects de la relativité de la normalité. (1) Il faut distinguer soigneusement les caractérisations des anormaux au point de vue de la symptômologie et du fonctionnement social satisfaisant. En termes des catégories absolues il faut s'attendre à trouver plusieurs types extrêmes anormaux parmi ceux qui au point de vue de cette société spéciale sont les plus éloignés de cette catégorie; ils fonctionnent cependant d'une façon très satisfaisante parce qu'ils ont le soutien de leur culture. Contrairement on sait bien que beaucoup de nos aliénés auraient fonctionné d'une façon satisfaisante dans des cultures plus simples. Les efforts pour arriver à des définitions statistiques de la normalité, en tant que les données dérivent d'une culture et d'une époque, offrent donc des renseignements sur cette civilisation et cette époque, non pas des descriptions de la anormalité dans un sens absolu. (2) Les formes épidémiques du comportement auxquelles les individus instables sont sujets sont choses des formes de culture comme tout autre comportement et l'on doit les étudier comme groupes d'habitudes. Où qu'on honore une forme spéciale de comportement qui touche à peu près à l'anormal, l'individu instable est sujet à cette anormalité indiquée et les autres formes peuvent être absentes, paraît-il.

BENEDICT

KULTUR UND DER ABNORMALE

(Referat)

Abnormalität ist, soweit sie sich auf veränderliche Organizationen der Persönlichkeit bezieht, kulturell bestimmt. Sie hat natürlich ihre organischen Determinanten, aber die Symbole sowohl wie die soziale Äusserung des von der natürlichen Gestalt Abweichenden sind relativ mit Bezug auf die Kultur. Die meisten dieser Persönlichkeitsorganisationen, die uns abnormal erscheinen, sind von verschiedenen Zivilisationen zur Grundlegung ihres institutionalen Lebens gebraucht worden, und Individuen, die diese Typen representieren betätigen sich in solchen Fällen leicht und ehrenhaft in der Gesellschaft. Umgekehrt sind die geschätzten Züge unserer normalen Individuen in anders organisierten Gesellschaften als Abweichungen betrachtet worden, und in solchen Fällen werden Personen mit diesen Zügen gesellschaftlich nicht unterstützt und betätigen sich nicht angemessen.

Es müssen zwei Gesichtspunkte hinsichtlich der Relativität von Normalität betont werden. (1) Das Charakterisieren von abnormal vom

Gesichtspunkt der Symptomologie und der sozialen Betätigung müssen sorgfältig unterschieden werden. Mit Bezug auf absolute Kategorien dürfen wir manche extrem abnormale Typen unter denjenigen erwarten, die vom Gesichtspunkt jener besonderen Gesellschaft am weitesten von dieser Kategorie entfernt sind; sie funktionieren aber höchst befriedigend, weil sie die Unterstützung ihrer Kultur haben. Umgekehrt ist anerkannt worden, dass wiele Insassen unserer Anstalten in einfachern Kulturen hinreichend funktioniert hätten. Bemühungen zu statistischen Definitionen der Normalität zu gelangen, insofern als sie ihr Material von einer Kultur und einer Epoche sammeln, geben Auskunft über die Zivilisation und das Jahrzehnt, aber keine Beschreibung der Normalität im absoluten Sinne. (2) Die epidemischen Formen des Verhaltens zu welchen unbeständige Individuen neigen, sind Angelegenheiten kultureller Formung, wie jedes andere Verhalten und müssen als Gewohnheitsbildungen untersucht werden. Wo eine besondere Form des Grenzverhaltens geehrt wird, neigt das unbeständige Individuum zur entsprechenden Besitzergreifung, und andere Formen mögen offenbar fehlen.

BENEDICT

THE EFFECT OF SIZE OF PUPIL ON THE FORM AND COLOR FIELDS*

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In previous papers (1-5, 8) the effect of the following factors on the size and shape of the form and color fields has been studied: intensity of stimulus, general illumination and state of adaption of the eye, difference in coefficient of reflection between stimulus and background, size of stimulus, brightness of preexposure and surrounding field, age, sex, and errors of refraction. It has been the purpose in the present paper to study the effect of size of pupil.

The type and amount of effect of size of pupil have long been a matter of conjecture. In addition to their interest for the general subject of perimetry, they sustain an important relation to applied perimetry. In clinical work it is important to know the effect in case both of drug and pathological miosis. In the treatment of glaucoma with pilocarpin, for example, the physician should know what allowance to make, if any, for the effect of the small pupil in interpreting the results obtained in an examination of the fields. In case of the Argyll-Robertson pupil also, and in all other cases of miosis from any cause whatsoever, lack of exact knowledge of the effect of size of pupil confuses the interpretation of the results of field study.

Two divisions have been made of the study, one on non-pathologic eyes, the other on pathologic eyes. The work on non-pathologic eyes alone will be reported in this paper.

Among the possibilities of an effect of decreasing the size of the pupil on the size of the form and color fields the following may be mentioned:

1. *A decrease in the intensity of light forming the image.* The amount of light condensed into the image varies inversely as the square of the diameter of the pupil. This effect would tend to de-

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crease the size of the field and to give the characteristic shape of the narrower fields.

2. *A change in the brightness relation between object and background.* For form stimuli the apparent difference in brightness between object and background is increased at low intensities of light, due probably to increase in physiological contrast; i. e., with a given difference in brightness between object and background the amount of contrast induced is increased as the intensity of the illumination is decreased. With small pupils, therefore, a white stimulus appears to stand out against its background in sharper contrast. This factor would tend to widen the form field. The case for color is more complicated. At the lower illuminations red and yellow tend to darken more rapidly as compared with a gray which matches them at higher illuminations, and blue and green to lighten. With small pupils, then, the color stimuli will not match in brightness the gray backgrounds on which they are placed for perimetry with the pupil of the size that would be produced by 7 ft. c. of light on the arc of the perimeter. The result of the unmatched stimulus and background is twofold: (*a*) There is induction across the color from the background which tends to lighten or darken it, as the case may be, and to reduce its saturation. The effect of this would be to narrow the fields. (*b*) The stimulus does not disappear into its background as the limit of sensitivity is approached, but appears as a light or a dark spot concerning the coloredness of which the observer is apt to be in doubt. This condition leads to inaccuracy of judgment and in the majority of cases tends to narrow the limits of the color field.

3. *An effect on the clearness of the image formed.* By giving a clearer image this effect would probably tend to give a wider field for a certain range of decrease in size of pupil. The effect of decrease of size of pupil on the clearness of the image is very striking when the definition given by the refractive system of the eye is defective. By decreasing the cross section of the cone of rays focussed into the image, it decreases in direct proportion the size of the diffusion circles given by a faulty focus and thus gives a closer approximation to the point image of each point in the object which good imagery requires. The imagery for the peripheral field is, of course, very faulty. Any displacement of the object from the principal axis of the refractive system causes the refractive error known as the as-

tigmatism of incidence. That is, the oblique incidence of rays of light on lens gives the effect of the addition of a weak plus sphere and a strong plus cylinder with its axis at right angles to the plane of incidence. So far as the refractive system is concerned, the effect is that of a high compound myopic astigmatism for the larger values of angle of incidence. This effect is modified, however, by the decrease in the distance of the percipient elements from the principal refracting plane of the optical system of the eye. The effect thus in a large number of eyes is to give a high mixed astigmatism for points in the field as far removed from the peripheral axis of the lens as are the limits of the form and color field.

With reference to this defect of imagery in the peripheral field, two of us in previous studies (6, 7) have found three types of eye: (*a*) a type in which the refractive error takes the form of a mixed astigmatism with a wide interval of Sturm; (*b*) a type in which the error is a compound hyperopic astigmatism with a narrower interval of Sturm; and (*c*) a type in which the refractive situation in the nasal and temporal halves of the field is asymmetric. In all of these types the effect is very great in those regions of the field important to perimetry.

Decrease in size of pupil also decreases the blurring due to spherical aberration and to other refractive irregularities in the peripheral portions of the lens and cornea. Running counter to the beneficial action of the decrease in the size of pupil, however, is the effect of diffraction on the clearness of the image. The apparent effect of diffraction is to cause the rays near the edge of the pupil to form faint circles or bands about each point in the image. The breadth of the entire diffraction pattern consisting of central disc and concentric rings sustains an inverse relation to the size of the pupil. Thus, so far as clearness of imaging is concerned, there is an optimum range of size of pupil above and below which imagery becomes poorer. The determination of the limiting values of this range has thus far been made with an artificial pupil which was in most cases located too far from the eye. It is difficult to predict just what the limiting values are with the natural pupil. However, determinations of the optimum size of pupil have recently been made in our laboratory with an artificial pupil in close proximity to the cornea and the natural pupil expanded under the influence of mydriatic.[1]

[1]This work was done by the two senior authors in collaboration with M. I. Smith.

The optimum size was found to be around 1.2-2 mm. in diameter. The size of the pupil under the influence of pilocarpin for our group of normal observers averages 2.25 mm. in diameter. The size of the Argyll-Robertson pupil found in a pathologic group which we have also studied in relation to the effects of miosis averages 2.42 mm. It is probable, therefore, that in both drug and pathological miosis we are working above the range of sizes in which diffraction dominates the effect on the image. It seems safe to assume in these cases, then, a beneficial action of the small pupil on the clearness of the image.

A still further consideration in this connection is a possible differential effect of type of eye. That is, a myopic eye is more myopic for blue than for red and it might be expected that the focussing action of the small pupil would increase visibility for blue more than for red. For the hyperopic eye, on the other hand, the defect in focussing is greater for red than for blue. In this case, therefore, the effect of the small pupil is in favor of red as compared with blue. For the presbyopic eye also, which is in effect hyperopic for the distance of the perimeter arc, 33 cm., the defect in focussing is greater for red than for blue and the effect of the small pupil would again tend to increase the extent of the red field as compared with the blue.

4. *A possible effect on the size of the field as a matter of geometrical projection.* A broad pupil, for example, tends to give a wider angle of projection of field on the retina and a narrow pupil the converse effect.

It is obvious that it is impossible to say prior to an experimental test just what is the net effect of factors such as are given above on the size and shape of the fields for form and color. In making such a test it is scarcely needful to state that an artificial pupil cannot be used to produce the small pupil needed for the test. While it is possible to use an artificial pupil with some degree of success for work in the central field, the breadth of the field is narrowed too much for it to be used in a determination of the limits. Furthermore, we need to know the effect of all the factors that are present when the miosis is induced by such drugs as pilocarpin and the mydriasis by such drugs as euphthalmin. It has been the purpose of this study to determine the effect of pilocarpin on the field for non-pathologic eyes.

Method of Working

The limits of the field were determined on the Ferree-Rand perimeter with strict observance of all the precautions prescribed for the use of this perimeter. The illumination at every point in the field was kept constant at 7 ft. c. Two form-field stimuli were used, 1- and 0.17-degree white on black, coefficients of reflection respectively 78 and 3.5 per cent, and two color stimuli, 1-degree blue and red.[2] The color stimuli were mounted on a gray background, coefficient of reflection 9.6 per cent, which was approximately the same brightness as the color at its limit of visibility. This background is supplied with the perimeter and was selected on the basis of determinations on a large number of observers. As has been noted in previous papers, however, it is impossible to procure an exact match for all observers and for all parts of the field. A brightness match between a color and a gray at some point in the peripheral field will not hold for all points in the field. Individual variations in the extent of the field in different meridians prevent, therefore, the possibility of obtaining one gray which will match a color as it appears at the limit of visibility for all meridians and all observers. The gray chosen meets this ideal most nearly for the greatest number of observers and meridians.

Eight non-pathologic subjects were used as observers: two emmetropes, three hyperopes, and three myopes. Two of them, one myope and one hyperope, were over 40 years of age, but neither needed a correction for the reading distance.

Two series of observations were made: (*a*) with the pupil of the size that was normal for the illumination on the arc of the perimeter, and (*b*) with the pupil contracted by two drops of a 2-per-cent solution of pilocarpin. In the latter case the work was not begun

[2]It was decided to omit the green stimulus in this part of the work for the following reasons: (*a*) The green field has been found by us to have little value in diagnosis based on size of field because of the wide scatter of results obtained for non-pathologic subjects. We find the green stimulus valuable as a stimulus of high sensitivity for scotometry in detecting localized disturbances in the central field, but unsatisfactory for use in diagnosis based on determinations of the limits of the field. (*b*) We were unable in the time allowed to map more than four fields satisfactorily. Since this was the case the four stimuli used were the ones considered the more valuable. The green stimulus was used, however, in case of one observer, G. R., in the analytical study that was made to determine the comparative influence of factors.

until the myopia which occurred for some of the observers immediately after the instillation of the miotic had worn off. Care was taken to measure the pupil repeatedly to insure that the effect of the miotic had not significantly diminished while the fields were being mapped. The size of the pupils in both series of experiments was measured with the Ferree-Rand pupillometer. The results obtained give data on the effect of the decrease in size of pupil on the size of the field for non-pathologic observers of the different refraction groups. Pilocarpin or some similar miotic had to be used to change the size of the pupil because it is not possible to employ an artificial pupil in the determination of the limits of the field. Furthermore, since an artificial pupil does not give the same effect as the natural pupil, its use is never advisable when it can be avoided. Even if possible, it would have been particularly inadvisable in the present case because (*a*) the effects involved are small, and (*b*) the clinical bearing of the results obtained made it practically imperative that the effects be determined for representative changes in the size of the natural pupil.

In order to separate from the total effect the influence of some of the more important contributive factors, an analysis was made for one myopic observer, G. R., and one hyperopic observer, L. S. For the former observer the stimuli used were 1- and 0.17-degree white on black and 1-degree red, blue, and green on gray of the brightness of the colors; for the latter observer they were 1-degree red and blue. The following points were determined:

1. The amount the fields were narrowed by the decrease of intensity of light in the image produced by the contraction of the pupil. To make this determination the fields were mapped with the normal pupil (*a*) with 7 ft. c. of light on the arc of the perimeter and (*b*) with the effective value of 7 ft. c. for the contracted pupil. The difference in size of field for these two determinations should be the amount the fields were narrowed by the decrease of intensity of light in the image produced by the contraction of the pupil, provided that there was no significant difference in the size of the pupil under the two intensities of illumination. For observer G. R. the normal size of the pupil when measured under 7 ft. c. of light on the arc of the perimeter was 4.6 mm. and under the action of pilocarpin 2.17 mm. The reduction factor for the pilocarpin pupil was therefore 4.5 and the effective or reduced value of the

illumination, 1.56 ft. c. The intensity of illumination on the arc of the perimeter was then decreased by means of layers of tissue paper until 1.56 ft. c. were obtained and the size of the pupil measured again. The breadth of the pupil was found still to be 4.6 mm., i. e., the difference in the amount of light reflected to the eye from the arc of the perimeter and its matched gray background in the two cases was not sufficient to produce a measurable change in the size of the pupil when the eye was accommodated for a distance of 33 cm. For L. S. the size of the normal pupil under 7 ft. c. on the arc of the perimeter was 6.15 mm.; under the action of pilocarpin it was 2.22 mm. The reduction factor was thus 7.7 and the effective value of 7 ft. c. on the arc was for the contracted pupil 0.9 ft. c. In case of this observer also no difference was found in the breadth of the pupil for 7 and 0.9 ft. c. on the arc of the perimeter when the eye was accommodated for a distance of 33 cm. As before, the measurements of the size of the pupil were made with the Ferree-Rand pupillometer. With this instrument measurements may be made with very high accuracy and precision.

2. The amount the fields were widened by the improvement in focussing produced by the contracted pupil. To make this determination the fields were mapped with the contracted pupil with 7 ft. c. of light on the arc of the perimeter and with the normal pupil with the amount of light on the arc of the perimeter which would give, as nearly as could be determined, the same intensity of light in the image as was had with the contracted pupil, namely, 1.56 ft. c. for G. R. and 0.9 ft. c. for L. S. The difference in the size of the fields for these two determinations should represent the amount the fields were widened by the better focussing of the smaller pupil.

3. The effect on the size of the color fields produced by the change in the brightness relation between the stimulus and background due to the decrease in the intensity of light in the image. To determine this effect the difference was taken between the sizes of field obtained with the normal and the contracted pupil with 7 ft. c. on the arc of the perimeter and with backgrounds which matched the colors in brightness for each condition of pupil. In both cases these backgrounds were carefully matched in each meridian at the limiting points in the field.

In addition, it was thought advisable to study the effect of pilo-

carpin with a more pronounced difference in the brightness relation between stimulus and background. For this black blackgrounds and preexposures were used. One point of interest in this determination is the information it gives concerning the amount of effect on the color fields which might be expected from the use of pilocarpin by the older methods of perimetry in which black was used as a background for all stimuli. Not only were these relations of stimulus to background used in determining the total effect of the pilocarpin but also in determining the effect of each of the factors when taken separately. While these latter determinations were not needed to complete the analysis, the results obtained are helpful in understanding the effect of the interplay of factors.

4. The difference in the effect of pilocarpin on the size of the color fields for hyperopic and myopic eyes. To determine this a comparison was made of the size of the fields for blue and red for the myopic observer G. R. and the hyperopic observer L. S. This comparison was made under all of the experimental conditions employed, namely, with both the normal and the contracted pupils with 7 ft. c. of light on the arc of the perimeter, with the normal pupil at the effective illumination present with the contracted pupil, and for all these conditions with the relations of stimulus to background noted in the preceding paragraph.

Results

The results obtained showing the effect of pilocarpin on size of field for eight non-pathologic subjects are given for the form fields in Table 1 and for the color fields in Table 2. In each table is given the observer's type of refractive error, diameter of normal pupil, diameter of contracted pupil, size of field for each stimulus estimated on the basis of the average breadth of field in the eight principal meridional quadrants, and the amount the size of field is changed by the action of the miotic.

The results given in these tables may be summarized as follows:

1. For 1-degree white there was a slight but consistent decrease in the size of the field for all observers. On the average this amounted to 1.4°. In computing the average for this and the following stimuli the index for size of field which we have adopted for use in our diagnostic scales was employed, namely, the average breadth of field in the eight principal meridional quadrants. The range was

TABLE 1

SHOWING A COMPARISON OF THE SIZE OF THE FIELD FOR 1- AND 0.17-DEGREE WHITE ON BLACK FOR THE NORMAL AND THE CONTRACTED PUPIL WITH 7-FT.-C. ILLUMINATION OF THE ARC OF THE PERIMETER

(8 non-pathologic observers)

Observer	Refraction	Diameter of normal pupil	Diameter of contracted pupil	Size of field (average meridian) 1-degree white		0.17-degree white		Amount size of field is changed by use of pilocarpin	
				Normal pupil	Contracted pupil	Normal pupil	Contracted pupil	1-degree white	0.17-degree white
M.I.S.	Emmetropia	6.0 mm.	3.1 mm.	70.8°	68.5°	51.1°	50.8°	—2.3°	—0.3°
M.E.	Emmetropia	4.0	2.75	70.6	69.5	55.3	54.9	—1.1	—0.4
L.S.	Hyperopia	6.15	2.22	73.6	71.0	54.5	49.5	—2.6	—5.0
M.S.	Hyperopia	5.2	2.2	67.1	65.8	51.5	50.5	—1.3	—1.0
G.M.	Hyperopia	4.0	1.5	74.0	73.6	59.9	58.0	—0.4	—1.9
G.R.	Myopia	4.6	2.17	71.3	70.7	57.3	56.9	—0.6	—0.4
E.L.	Myopia	6.7	1.95	67.3	65.4	55.9	50.6	—1.9	—5.3
D.N.	Myopia	5.0	1.33	73.0	72.2	49.8	44.6	—0.8	—5.2
Average		5.21	2.15	71.0	69.6	54.4	52.0	—1.4	—2.4

TABLE 2

SHOWING A COMPARISON OF THE SIZE OF THE FIELD FOR 1-DEGREE BLUE AND RED ON GRAY OF THE BRIGHTNESS OF THE COLOR FOR THE NORMAL AND THE CONTRACTED PUPIL WITH 7-FT.-C. ILLUMINATION OF THE ARC OF THE PERIMETER
(8 non-pathologic observers)

Observer	Refraction	Diameter of normal pupil	Diameter of contracted pupil	Size of field (average meridian) 1-degree blue Normal pupil	1-degree blue Contracted pupil	1-degree red Normal pupil	1-degree red Contracted pupil	Amount size of field is changed by use of pilocarpin 1-degree blue	1-degree red
M.I.S.	Emmetropia	6.0 mm.	3.1 mm.	46.0°	42.4°	37.0°	34.6°	— 3.6°	—2.4°
M.E.	Emmetropia	4.0	2.75	43.0	35.8	39.0	34.3	— 7.2	—4.7
L.S.	Hyperopia	6.15	2.22	47.5	41.0	44.6	45.3	— 6.5	+0.7
M.S.	Hyperopia	5.2	2.2	46.1	41.6	39.8	37.8	— 4.5	—2.0
G.M.	Hyperopia	4.0	1.5	43.1	32.8	40.6	43.6	—10.3	+3.0
G.R.	Myopia	4.6	2.17	42.1	36.8	40.0	37.1	— 5.3	—2.9
E.L.	Myopia	6.7	1.95	49.0	37.1	35.9	32.0	—11.9	—3.9
D.N.	Myopia	5.0	1.33	40.8	32.0	31.9	29.0	— 8.8	—2.9
Average		5.21	2.15	44.7	37.4	38.6	36.7	— 7.3	—3.1 +1.9

from 0.4° to 2.6° for the different observers. For the practical purposes of perimetry the amount of decrease for this stimulus can be considered negligible for non-pathologic subjects.

2. For 0.17-degree white there was a slightly greater decrease in the size of the field for all observers. On the average, it amounted to 2.4°. The range was from 0.3° to 5.3° for the different observers. For three of the eight observers the decrease was significant, amounting to 5.3°, 5.2°, and 5.0° respectively. Two of them were myopes and one a hyperope. For the other observers the decrease in the field was probably insignificant.

3. For 1-degree blue the field was significantly smaller for all observers. On the average, the reduction amounted to 7.3°. For the different observers it ranged from 3.6° to 11.9°. It would seem from this result that if the field for blue is found to be contracted in case of a patient with a very small pupil, allowance should be made in interpreting the result for the fact that normally the blue field is contracted by the conditions presented by this size of pupil.

4. For 1-degree red the size of the field was decreased by the small pupil for six of the observers and increased for two. The average decrease for the six subjects was 3.1° and the range from 2.0° to 4.7°. The widening of the field for one of the two subjects for which an increase occurred was 0.7°, and for the other, 3.0°; average, 1.9°. Both of these observers were hyperopes, the value of the hyperopia for the latter being 3.75 D. This observer was also approaching presbyopia. It is interesting to note that for both of these observers the blue fields were larger than the red fields by about 3° when both were mapped with normal pupils. Under the influence of pilocarpin, however, the red fields were larger than the blue by 4° for one observer and 11° for the other.

The results of the analysis of the factors contributing to the effect of pilocarpin on size of field as discussed in the section on method of working is given in Table 3. Column 1 of this table gives the observer; column 2, the stimulus used; column 3, the size of field for the normal pupil with 7-ft.-c. illumination on the arc of the perimeter; column 4, the size of field for the contracted pupil with 7 ft. c. of light on the arc of the perimeter and an effective illumination of 1.56 ft. c. for the eye (observer R) and 0.9 ft. c. (observer S); and column 5, the size of field for the normal pupil with a value of illumination on the arc of the perimeter equal to these

TABLE 3

SHOWING THE RESULTS OF AN ANALYSIS OF THE FACTORS CONTRIBUTING TO THE EFFECT OF PILOCARPIN ON THE SIZE OF THE FORM AND COLOR FIELDS

Factors considered are (1) the reduction of the amount of light entering the eye; (2) the better focussing action of the small pupil; (3) the change of brightness relation between color stimulus and its background; and (4) the differential effect of type of eye.

Observer	Stimulus	Size of field (average meridian) Normal pupil at 7 ft. c.	Contracted pupil at 7 ft. c.	Normal pupil at equivalent illumination	Amount size of field is changed by use of pilocarpin	Effect of reduction in intensity of light	Effect of better focusing action of small pupil
G.R. (Myope)	1 ° White	71.3°	70.7°	68.6°	—0.6°	— 2.7°	+2.1
	0.17° White	57.3	56.9	54.9	—0.4	— 2.4	+2.0
			With perimeter backgrounds				
G.R. (Myope)	1 ° Blue	42.1	36.8	30.7	—5.3	—11.4	+6.1
	1 ° Red	40.0	37.1	31.8	—2.9	— 8.2	+5.3
	1 ° Green	23.3	21.9	17.8	—1.4	— 5.5	+4.1
L.S. (Hyperope)	1 ° Blue	47.5	41.0	39.8	—6.5	— 7.7	+1.2
	1 ° Red	44.6	45.3	41.9	+0.7	— 2.7	+4.4
			With matched backgrounds				
G.R. (Myope)	1 ° Blue	42.8	38.3	32.1	—4.5	—10.7	+6.2
	1 ° Red	41.9	39.0	34.5	—2.9	— 7.4	+4.5
	1 ° Green	26.9	25.1	20.1	—1.8	— 6.8	+5.0
			With black backgrounds				
G.R. (Myope)	1 ° Blue	39.1	32.7	30.2	—6.4	— 8.9	+2.5
	1 ° Red	37.6	31.1	30.6	—6.5	— 7.0	+0.5
	1 ° Green	20.4	12.1	11.1	—8.3	— 9.3	+1.0

effective values. The difference in result between columns 3 and 4 represents the total effect of pilocarpin on size of field. This is shown in column 6. The difference in result between columns 3 and 5 represents the effect of the factor of reduced illumination. This is shown in column 7. The difference in result between columns 4 and 5 represents the effect of the improvement in focussing produced by the small pupil. This is shown in column 8. A comparison of the results under the divisions of the table headed "perimeter backgrounds" and that headed "matched backgrounds" shows the effect of the change in the brightness relation between stimulus and background caused by the reduction of the illumination by the contracted pupil, and a comparison of the results with gray and black backgrounds shows the effect of pilocarpin for a greater difference in brightness between stimulus and background and also gives an estimate of the effect of pilocarpin on size of field with the older technique of perimetry, i.e., perimetry with black surroundings. And, finally, a comparison of the size of the red and blue fields for the two observers, one a myope and the other a hyperope, shows the differential effect of type of eye.

From the results shown in this table the following points may be summarized:

1. For both form stimuli used, the widening of the field due to the focussing action of the small pupil was only slightly less than the contraction produced by the decrease in the intensity of light in the image, the net or combined effect being only a small reduction in the size of the form field.

2. For the color fields, however, the decrease in intensity of light was in general a more important factor than the improvement in focussing. Exceptions to this were the results for the red stimulus for the two hyperopic observers.

3. For the color stimuli there was a detectable differential effect of improvement due to the focussing action of the small pupil for type of refractive error. For the myopic observer the blue field was widened more by this factor than the red, and for the hyperopic observer the red field more than the blue. These effects correspond to what might be expected as the result of the comparative amounts of benefit from the focussing action of the small pupil for red and blue in these two types of eye. In this connection it must be re-

membered that presbyopes must be classed as hyperopic for the distance of the stimulus on the arc of the perimeter.

4. The contraction was less for the colored stimuli when the backgrounds were carefully matched to the stimulus at each limiting point in the field for the contracted pupil than was the case when the grays which matched the colors for the normal size of pupil under 7 ft. c. of illumination were used. With the still greater deviation from an exact match presented by the black background correspondingly greater contractions were obtained. The effect of pilocarpin on the size of the color fields is thus minimized by using backgrounds which match the stimuli in brightness and exaggerated by the use of a black background.

5. When matched backgrounds were used throughout the series the blue field was contracted most, the red next, and the green least by the reduced illumination produced by the small pupil. For the black background the order of amounts of contraction due to this factor was green, blue, and red.

Summary

In the foregoing paper results are given for the effect of pilocarpin on the size of the form and color fields for non-pathologic eyes. The form stimuli used were 1-degree and 0.17-degree white on black background and the color stimuli were 1-degree red and blue on gray backgrounds of the brightness of the color. Emmetropic, myopic, and hyperopic observers were used. The results obtained are discussed in relation to the effect of pilocarpin on the size of the pupil. A statement is given of the possibilities presented by size of pupil for affecting the size of the form and color fields and experimental results are given showing the comparative importance of each of these possibilities.

The following points may be noted:

1. *Total effect*

a. For the 1-degree white stimulus there was a slight but consistent contraction of the field for all observers amounting on the average to 1.4 degrees. The range for the different observers was from 0.4 to 2.6 degrees.

b. For the 0.17-degree white stimulus there was a slightly greater contraction of the field for all observers. The amount on

the average was 2.4 degrees. The range for different observers was from 0.3 to 5.3 degrees. For three of the observers the average amounts of contraction were respectively 5.3, 5.2, and 5 degrees. For the other five observers the amount of contraction was probably not significant in relation to the practice of perimetry.

c. For the 1-degree blue stimulus the contraction was significant in amount for all observers. The average was 7.3 degrees; the range for the different observers was from 3.6 to 11.9 degrees.

d. For the red stimulus the field was contracted for six and widened for two of the observers. The average of the contraction for the six observers was 3.1 degrees and the range was from 2.0 to 4.7 degrees. The amount of widening for one of the two observers was 0.7 degree and for the other, 3.0 degrees. Both of these observers were hyperopes, the value of the hyperopia for the latter being 3.75 diopters. The latter observer was also in the early stages of presbyopia. For both of these observers the blue fields were larger than the red fields by about 3 degrees when the fields for both were mapped with normal pupils, while under the action of pilocarpin the red fields were larger than the blue by 4 degrees for one observer and 11 degrees with the other.

2. *Analysis of effect*

a. For both form stimuli the widening of the field due to the focussing action of the small pupil was only slightly less than the contraction produced by the decrease in the intensity of light in the image, the net or combined effect being only a small reduction in the size of the fields.

b. For the color fields, however, the decrease in intensity of light caused by the small pupil was in general a more important factor than the improvement in focussing. Exceptions to this statement are the results for the red stimulus for the two hyperopic observers.

c. For the color stimuli there was a detectable differential effect of improvement due to the focussing action of the small pupil for type of refractive error. For the myopic observer the blue field was widened more by this factor than the red, and for the hyperopic observer the red field more than the blue. These effects correspond to what might be expected as the result of the comparative amounts of benefit from the focussing action of the small pupil for red and

blue in these two types of eye. In this connection it must be remembered that presbyopes must be classed as hyperopic for the distance of the stimulus on the arc of the perimeter.

d. The contraction was less for the colored stimuli when the backgrounds were carefully matched to the stimulus at each limiting point in the field for the contracted pupil than was the case when the grays which matched the colors for the normal size of pupil under 7 ft. c. of illumination were used. With the still greater deviation from an exact match presented by the black background, correspondingly greater contractions were obtained. The effect of pilocarpin on the size of the color fields is thus minimized by using backgrounds which match the stimulus in brightness and exaggerated by the use of a black background.

c. When matched backgrounds were used throughout the series the blue field was contracted most, the red field next, and the green field least by the reduced illumination produced by the small pupil. For the black background the order of contraction due to this factor was green, blue, and red.

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L'EFFET DE LA GRANDEUR DE LA PRUNELLE SUR LES CHAMPS DE FORME ET DE COULEUR

(Résumé)

Dans cette étude on a fait une détermination de l'effet de la grandeur de la prunelle sur les champs de forme et de couleur. On a fait deux divisions de l'étude,—l'une sur les yeux non pathologiques, l'autre sur les yeux pathologiques. Dans cet article on ne donne que l'effet sur les yeux non pathologiques. Les stimuli de forme employés ont été d'un degré et de 0,17 d'un degré de blanc sur noir et les stimuli de couleur ont été d'un degré de rouge et de bleu sur des fonds gris de la clarté des couleurs. On a obtenu des résultats pour huit observateurs non pathologiques: 3 myopes, 3 hyperopes et 2 emmetropes. On décrit les possibilités présentées pour la grandeur du stimulus influant sur la grandeur des champs de forme et de couleur et on a fait une détermination expérimentale de l'importance comparative de ces possibilités. Les possibilités considérées sont: une décroissance de l'intensité de la lumière qui forme l'image; un changement de la relation de clarté entre l'objet et le fond; un effet sur la clarté de l'image; la condition réfractive de l'oeil; et un effet sur la grandeur du champ comme matière de projection géométrique.

Ferree, Rand, et Sloan

DIE WIRKUNG DER PUPILLENGRÖSSE AUF FORM- UND FARBENFELDER

(Referat)

In dieser Untersuchung wurde die Wirkung der Pupillengrösse auf die Form- und Farbenfelder bestimmt. Man teilte die Untersuchung in zwei Abteilungen, die der nichtpathologischen, und die der pathologischen Augen. In dieser Arbeit wird nur die Wirkung auf nichtpathologische Augen gegeben. Die gebrauchten Formreize waren 1 Grad und 0,17 Grad weiss auf schwarz, und die farbigen Reize waren 1 Grad rot and blau auf grauen Hintergründen, die die Helligkeit der Farben hatten. Man erhielt die Ergebnisse von 8 nichtpathologischen Beobachtern: 3 kurzsichtigen, 3 weitsichtigen und 2 normalsichtigen. Man berichtet über die Möglichkeiten bezüglich der Wirkung der Reizgrösse auf die Grösse der Form- und Farbenfelder, und man bestimmt die vergleichende Bedeutung dieser Möglichkeiten experimentell. Die in Betracht gezogenen Möglichkeiten sind: eine Abnahme der Lichtstärke, die das Bild herstellt; eine Veränderung im Helligkeitsverhältnis zwischen Objekt und Hintergrund; eine Wirkung auf die Klarheit des Bildes; die Brechungsbedingungen des Auges; und eine Wirkung auf die Feldgrösse als geometrische Projektion.

Ferree, Rand, und Sloan

ON THE DETECTION OF VARIATIONS IN TEMPO OF SPEECH BY VISUAL, TACTUAL, AND VISUAL-TACTUAL CUES*[1][2]

From the Psychological Laboratory of Northwestern University

MILLA L. ILIEVA

Experiments carried on in recent years in the Laboratory of Psychology in Northwestern University under the direction of Dr. Robert H. Gault have shown that the deaf are able to enlarge the use of their sense of touch as a means of finding cues to help them interpret spoken language: that tactual stimulation from spoken language when received simultaneously with visual stimulation as in lip-reading is an important auxiliary to the latter in relation to the interpretation of speech (2). The deaf are able to locate the accented words in sentences, detect changes of rhythm, and discriminate among vowel and certain consonantal qualities. The results of the work done heretofore have shown that some among the above-mentioned characteristics of speech are discriminated through tactual stimulation much better than through lip-reading alone,[3] and therein are some of the reasons for the advantage of double (tactual-visual) stimulation.

STATEMENT OF THE PROBLEM

The purpose of the present investigation is to determine the relative ability of the eye alone, of the finger-tip alone, and of both together to detect differences in the tempo of speech within spoken

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[1]An investigation carried on at Northwestern University by aid of the Barker Foundation, under the direction of Robert H. Gault.

[2]Abstract of a thesis presented to Northwestern University in June, 1932, in partial fulfillment of the requirements for the degree of Master of Arts. The work is a part of the general program of research that is being carried on by Robert H. Gault and has been done under his direction.

[3]For material bearing directly on this point, see, e.g., Gault (1) and Weichbrodt (5); for material bearing less directly, but importantly, on the point see Goodfellow (3).

sentences and to locate the quickly spoken words. It is possible that herein we may find an additional source of the superiority of tactual-visual over visual stimulation alone, in addition to the sources already mentioned. Moreover, we want to find what patterns (in respect to the distribution of tempo) are most accurately detected. The three methods used are those of lip-, touch-, and lip-touch reading.

Apparatus and Controls

The instrument now used in the Psychological Laboratory of Northwestern University for the transmission of the stimuli to the tactual organs is the new Gault-Teletactor (analogous to a telephone system) through which the vibrations corresponding to speech are communicated by a powerful low-impedence magnetic vibrator. Speech sounds of between 200 and 8000 vibrations per second are transmitted from the speaker's microphone through the amplifier to the vibrator or receiver unit with practically no distortion. To eliminate the possibility of auditory stimulation of hearing subjects the instrument is suspended by springs within a double concrete box with a padded opening to admit the subject's arm and to enclose it snugly when his fingers are resting upon the vibrating unit. The box is a cube, 34 inches outside measurement. There are 3 inches between the inner and the outside boxes. The inner one is lined with cotton batting, and the whole weighs about 1500 pounds.

At all times the experimenter stood with her microphone in a sound-deadening room behind a double glass window which was covered when tactual stimuli alone were being presented and uncovered when either visual stimuli alone or simultaneous visual and tactual stimuli were being presented. Moreover, a sound barrier (64-cycle source) was applied through ear phones. Repeated tests gave satisfactory proof that by all these means hearing was eliminated.

Subjects

There were 33 subjects, students at Northwestern University, whose chronological age ranged from 19 to 22 years; in addition, there were two who were over 22 years of age. The 33 subjects were completely naïve; they had not done any work on the teletactor or in lip-reading before. The remaining two had had some

experience with the teletactor and in lip-reading. They had served as subjects in the laboratory in other experiments of this nature. All subjects were people with practically normal hearing.

The work exacted a high degree of attention and patience and the cooperation given to me by all subjects is very deeply appreciated.

Stimuli

The stimuli presented consisted of 40 sentences of five different patterns in respect to tempo, eight of each kind. There were quickly spoken words at different places in the sentences.

1. Sentences with quickly spoken words in the first part.
   Pattern: . . . - - - - -
2. In the last part.
   Pattern: - - - - - . . .
3. Somewhere between the two extremes.
   Pattern: - - - . . . - - -
4. Both in the first and last parts.
   Pattern: . . . - - - . . .
5. Sentences with two quickly spoken phrases in the latter part.
   Pattern: - - - . . . . . .[4]

The sentences were made of monosyllabic words. Thus the subjects could count the words in the sentence by the "punches" felt on their finger-tips. These were in most cases prepositional phrases consisting of two or three words and are indicated above by periods. The more slowly spoken phrases are indicated by hyphens. All were colloquial English words. The average number in a sentence was seven.

A brief analysis of the frequency of the consonantal sounds in the words and the frequency of the words constituting the sentences is important and interesting. The analysis given in Table 1 shows the number of times each consonantal sound occurs.[5] This analysis has been compared and correlated with an analysis of consonantal

[4]In the example of the various patterns, the dots represent the quickly spoken words and the dashes the rest of the words in the sentences.

[5]In computing frequencies of sounds each word was analyzed as follows: e.g., the word "last" appeared thus:

Word	Frequency of word	Sound analysis
last	2	L-2; S-2; T-2

TABLE 1

Sound	Frequency	Percentage
1. T	50	13.22
2. D	35	9.26
3. N	42	11.11
4. P	4	1.06
5. B	6	1.59
6. M	19	5.03
7. F	25	6.61
8. V	3	0.79
9. S	31	8.20
10. Z	13	3.44
11. Th	10	2.65
12. Sh	18	4.76
13. Ch	6	1.59
14. K	12	3.18
15. G	17	4.50
16. L	27	7.14
17. R	33	8.73
18. H	27	7.14
Sum*	378	100.00

*Dewey's analysis shows that: P, B, T constitute 25 per cent of all our sounds; P, B, T, D, K, G, F, V, Th constitute 50 per cent of all our sounds; P, B, D, K, G, F, V, Th, S, Z, Sh, Ch, J, M, N, Ng constitute 75 per cent of all our sounds. The reason the frequency of the sounds T, D, and N is so great in comparison to P B, and Ch here is that the first group (T, D, N) occurs in most of the two- or three-letter words found in the prepositional phrases so frequently used in the sentences.

sound-frequencies that has been made by Yale S. Nathanson (4). Using the following formula $1R = 1 - \frac{\Sigma 6D}{n(n^2-1)}$, a correlation of 0.737 ± 0.1076 was found between the frequency of consonantal sounds occurring in the stimuli sentences used in this study and the frequency of consonantal sounds occurring generally.

In these sentences 48 per cent of the sounds were vowels as compared with 38 per cent in general speech.

Examples of Stimuli

1. By-the-way, will you come with me?
2. As-soon-as I saw her, I-knew-her.
3. We will get there by-and-by.

4. I-wish you would see part-of-it.
5. He built his hut on-the-lake shore.
6. How far, do-you-think, she has gone?
7. If I had known-of-it, I would have stopped her.
8. She knew of-it, yet she went.
9. She heard his song at-the-end of-each-day.
10. The nest is on-the-top of-the-tree.

Method

Presentation of Stimuli. As a preliminary, the experimenter practiced at pronouncing the sentences until she was confident of her ability to utter them again and again with reasonable uniformity. Had it been practicable in all the circumstances, it would have been desirable to present them from a vocal record and from a moving film respectively, but for the fact that it was our purpose to keep as close as possible to the conditions of normal speech.

As we have already indicated, the stimuli were presented in three different ways: visually, for straight lip-reading; tactually, for feeling the vibration produced by the words on the teletactor; and visually and tactually, for lip-touch reading. The sentences were read as presented above, that is, the different sentence patterns were scattered. They were not presented in a uniform order. All forty were presented three times during a single sitting, once for each of the visual, tactual, and visual-tactual readings. The order of presentation to the different sense modalities for all subjects during the three one-hour sittings was as follows:

1st: visual, tactual, visual-tactual.
2nd: tactual, visual-tactual, visual.
3rd: visual-tactual, tactual, visual.

The experimenter always rang a buzzer to indicate that a stimulus was about to be presented. In the visual and in the dual-stimulation series the experimenter waited for about three seconds after the subjects had looked up so that he would get a normal eye fixation before she read the sentence. Not once did the subjects receive any aid or clues to the sentences presented, except from the vibration of the diaphragm on the teletactor or the moving lips on the otherwise expressionless face of the experimenter or from both combined. In the touch situation, as already explained, the subject had his

forearm in a cement sound-proof box in the middle of which hung the teletactor. The manner of reading and recording the stimuli (sentences) demanded a great deal of concentration and attention to the visual and visual-tactual stimulation. Each subject felt the vibration on the diaphragm during the tactual series and tried at the same time to remember the pattern of the sentence as it was pronounced. The subjects were required to record the pattern they received thus:

1. Writing a dash for each of all but the quickly spoken words.
2. Indicating each quickly spoken word by a single dot.

Results

Tables 2 and 3 represent the results of this experiment.

A uniform increase of accuracy of judgments in the three sittings is very obvious. Thus the total score of correct reports for sitting I (vision) is 748 out of 1400; for sitting II it is 840 out of 1400; and the total of correct reports for sitting III is 925 out of 1400. A similar uniform increase is observed in the results from the tactual and visual-tactual situations. (See Table 2.) These are effects of practice.

An increase of correct reports in the same sitting, in respect to different methods of stimulation, is found also. In the visual series the total of correct reports in the first sitting is 748 out of 1400; in the same sitting the correct reports based upon tactual stimulation alone is 812 out of 1400, and in the visual-tactual it is 925. (See Table 2 for corresponding figures relating to the second and third sittings.)

It is interesting to notice that the correct reports are fewest in relation to the third pattern (see above) in all sittings and methods of presentation, with the exception of the visual series in the first sitting. (See Table 3.)

The group results as shown in the table above are important and interesting. The highest total score of correct reports is found by the visual-tactual method, with the tactual method following second, and visual last. The percentage of correct reports is distributed more or less evenly though there is a greater jump from visual to tactual than from tactual to visual-tactual. The increase in learning or the effect of practice is greater for the tactual than for either of the

TABLE 2
PRACTICE EFFECT SHOWN BY GROUP SCORES

	Total number of stimuli	Sitting I		Sitting II		Sitting III			
		Total score of correct judgments	Percentage right	Total score of correct judgments	Percentage right	Total score of correct judgments	Percentage right	Increase of III over I	Increase of III over I in percentages
Vision	1400	748	53.5	840	60.0	925	66.0	12.5	23.3
Touch	1400	812	58.0	925	66.0	1040	74.4	16.4	28.3
Vision-touch	1400	925	66.0	1009	71.5	1084	77.2	11.2	17.2

TABLE 3
THE MEAN NUMBER OF RIGHT REPORTS FOR EACH METHOD, PATTERN, AND SITTING

Sitting	Vision Av.	Vision S.D.	Touch Av.	Touch S.D.	Vision-touch Av.	Vision-touch S.D.
			Pattern I			
I	3.9	1.4	4.4	1.1	5.2	1.3
II	4.9	1.1	5.6	1.1	5.8	1.4
III	5.6	0.9	6.4	1.4	6.6	0.9
			Pattern II			
I	4.1	1.5	4.6	1.2	5.2	1.2
II	4.7	1.3	5.4	1.3	6.0	0.9
III	5.5	1.0			6.3	1.1
			Pattern III			
I	4.0	1.1	4.2	1.2	4.8	1.3
II	4.2	1.2	4.5	1.3	5.0	0.8
III	4.7	0.9	4.8	1.2	5.4	1.1
			Pattern IV			
I	4.8	1.2	5.4	0.9	5.6	1.2
II	5.0	1.1	5.6	1.3	6.1	1.0
III	5.2	1.2	6.1	1.2	6.4	1.1
			Pattern V			
I	4.3	1.2	4.4	1.3	5.2	1.4
II	5.0	1.1	5.0	1.0	5.7	1.0
III	5.3	1.0	6.0	1.2	6.1	1.0

other two methods. It is clear that in this experiment, and in our conditions, the best results in respect to the detection and localization of heightened tempo in speech are achieved by means of dual stimulation. Inasmuch as a variation in tempo is one element in the whole pattern or "melody" of spoken language, which pattern is a carrier of meaning, it may be inferred from the results of this experiment that tactual sensation may be a material auxiliary to the lip-reader's eye in relation to the interpretation of speech.

Moreover, this result strengthens the hypothesis that the tactual sense may be made to serve as an aid in relation to the improvement of the speech of the deaf.

CONCLUSIONS

How does the ability of vision to detect the quickly spoken words in a sentence compare with that of touch and vision-touch? What tempo patterns are most accurately detected? These were the questions asked in the beginning of the experiment. The interpretation of the data and the results indicate that:

1. When subjects feel speech in their finger-tips through the teletactor and at the same time see it upon the face of the experimenter the combined impression enables them to detect the quickly spoken words in a sentence more accurately than does the visual impression alone, as it is found in straight lip-reading.

2. The percentage of correct judgments increases from 66.0 per cent in the visual situation to 77.2 per cent in the visual-tactual situation.

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SUR LA DÉCOUVERTE DES VARIATIONS DE LA VITESSE DE LA PAROLE AU MOYEN DE REPÈRES VISUELS, TACTILES, ET VISUELS-TACTILES

(Résumé)

Cette enquête représente une section d'un programme compréhensif de recherches dirigé par le professeur Robert H. Gault de Northwestern University. Il a rapporté que les lecteurs de lèvres sourds qui voient simultanément la figure d'un parleur et sentent ses mots au moyen du Teletactor Gault interprètent la parole beaucoup mieux que s'ils observent la figure sans sentir les mots prononcés.

Le problème présent est un parmi plusieurs qui ont comme but commun la découverte des sources spéciales des avantages de la stimulation double.

La forme ou la mélodie de la parole est un facteur qui aide à donner sa signification. Un élément de la forme est une vitesse variante.

Il s'agit ici de l'exactitude avec laquelle le sens de vision seul (du lecteur de lèvres), le sens de toucher seul (les organes tactiles du doigt restent sur la partie vibrante du Teletactor), et les deux sens ensemble peuvent découvrir les portions rapidement parlées de diverses phrases.

On a composé quarante phrases comme stimuli. L'expérimentateur les a

prononcées comme exercice préliminaire, jusqu'à ce qu'il a pu les prononcer avec un minimum de variation de temps en temps.

Les portions rapidement parlées ont été le mieux découvertes dans la condition de stimulation double, ensuite par le toucher seul, et le moins précisément par la vision seule.

ILIEVA

ÜBER DIE ENTDECKUNG DER VARIATIONEN IM ZEITMASS DER SPRACHE, BEI GESICHTS- UND TAST-, UND BEI GESICHTSTASTKENNZEICHEN

(Referat)

Diese Untersuchung stellt ein Ausschnitt eines umfassenden Untersuchungsprogramms dar, die Professor Robert H. Gault an der Northwestern University leitet. Er hat berichtet, dass taube Lippenleser, die gleichzeitig das Gesicht des Sprechenden sehen und seine Wörter durch den Gault-Teletactor fühlen, Gesprochenes bedeutend besser deuten als bei blossem Beobachten des Gesichts, ohne das Fühlen der gesprochenen Wörter.

Das gegenwärtige Problem ist eines von mehreren, die zusammen die besonderen Quellen der vorteilhaften doppelten Reizung finden wollen.

Der Styl oder die Melodie des Gesprochenen ist ein Faktor, der dessen Bedeutung übermittelt. Ein Element des Styls ist das varierende Zeitmass.

Die Frage hier lautet, wie genau kann der Gesichtssinn (des Lippenlesers) allein, der Tastsinn (die Tastorgane in den Fingern, die auf dem vibrierenden Teletactor ruhen) allein, und beide Sinne zusammen die rasch gesprochenen Teile verschiedener Sätze entdecken.

Die Reize waren vierzig Sätze. Der Experimentator übte zuvor das Sprechen der Sätze, bis er sie von Fall zu Fall mit einem Minimum von Variation sprechen konnte.

Die rasch gesprochenen Teile wurden am besten under der Bedingung der doppelten Reizung entdeckt, dann am nächstbesten durch den Tastsinn allein, und am wenigsten genau durch den Gesichtssinn allein.

ILIEVA

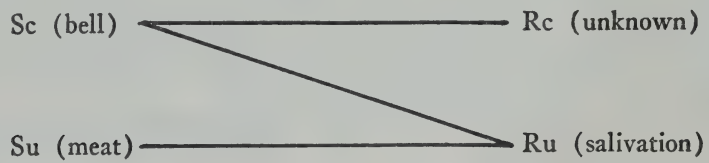
THE CONDITIONED REFLEX AS THE EXPLANATION OF HABIT FORMATION: I. THE ESSENTIAL FACTORS IN THE ESTABLISHMENT OF THE CONDITIONED REFLEX*

From the Department of Education of The Johns Hopkins University

J. M. Stephens

This experiment[1] was designed to find out the relative rôles of the difference in dominance and of the time interval in determining (*a*) the occurrence of the conditioned reflex and (*b*) the direction of the conditioning.

In the typical textbook discussion of the conditioned reflex the following illustration is given:



In this illustration the diagonal illustrates that the bell comes to evoke salivation. When the bright student asks why the other diagonal is omitted—why the meat does not elicit the response to the bell—he is told that the meat-salivation reflex is more dominant than the reflex originally initiated by the bell, and that this superior dominance determines the direction of the conditioning.

Upon reading farther, however, we find that another factor must be introduced, namely, that the bell must be presented before the meat. This factor of time difference is usually passed over lightly as

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[1]The investigation was made possible (*a*) by equipment borrowed from Dr. Dunlap and Dr. Dorcus of the Department of Psychology and from Dr. Hubbard of the Department of Physics, and (*b*) by a grant from the Department of Education.

In addition I have received much advice from Dr. Dorcus with regard to the apparatus. This, of course, does not imply his approval of the set-ups used.

being of little theoretical importance. It is altogether possible, however, that this time interval in itself would account for the direction of the conditioning, i.e., would account for the fact that the bell comes to cause salivation but that the meat will not elicit the response to the bell. If the time interval is the important thing, the explanation of the conditioned reflex immediately conforms to the law-of-effect schema. I have attempted to expound this at some length elsewhere (3, 4) and need merely state the law-of-effect schema as follows: If any situation, A, is habitually succeeded by any other situation, B, then situation A will come to call forth such a response as will, in combination with situation B, produce a biologically valuable condition. That is, if the sound of a certain voice is habitually followed by a kick on the buttocks, the sound of that voice will come to result in such a response as will remove the buttocks from the vicinity of the potential kicker. Here the absence of buttocks is considered as a biologically valuable condition. Similarly if the sound of another voice is habitually followed by the appearance of food at a certain point that voice will tend to call forth a response resulting in the appearance of the organism at the given place.

Such a law would explain the conditioned reflex and in doing so ask no more of dominance than that the second stimulus must be of such a nature that some responses will produce a more valuable combination with it than other responses will. That is to say, if the second stimulus had no biological significance but consisted merely of a cloud in the sky or the playing of a flat rather than a sharp, then it would not influence the connection between the preceding stimulus and the response to that stimulus. This modified law of effect would insist that this second stimulus must have some biological significance. Such an insistence is, however, very different from the demand that this second stimulus shall initiate a more dominant reflex than that originally initiated by the first stimulus. According to the law of effect the second reflex may have more, or equal, or less dominance provided that the stimulus itself has biological significance. The conditioned reflex, on the other hand, makes the question of relative dominance paramount and asks, only secondarily or incidentally (as far as the theory is concerned), that there shall be a time interval between the presentation of the conditioned and unconditioned stimulus.

The suspicion that time order in itself is sufficient to account for the conditioned reflex receives support from different sources. First, there is the long series of experiments[2] testifying to the fact that the conditioned stimulus must precede—that a time interval is necessary. Then there is the work of Wendt (5), showing that a blow on one patellar tendon preceding a blow on the other patellar tendon will come to evoke a response in the latter—that we can have definite uni-directional conditioning with no difference in dominance.

This study attempts to supplement such scattered evidence by making comparisons within one field of stimulation and by definitely varying the amount of dominance difference within a constant state of time interval and vice versa. It attempts to do this by setting up four conditions (hereinafter called "groups" in order to escape the confusion between an experimental "condition" and the phenomenon of "conditioning") as follows:

a. no time interval, no dominance difference,
b. no time interval, a definite dominance difference,
c. a definite time interval, no dominance difference,
d. both time interval and dominance difference; and to see in which groups, if any, conditioning will result, and which direction it will take.

Before proceeding to a more detailed description of the experiment I should indicate that the results are not as clear cut or as complete as one would wish. The incompleteness is due to the difficulty of controlling dominance difference and to the few subjects for whom complete records were obtained. I had hoped to obtain a much clearer picture of whatever facts there may be but, having long since exhausted my funds and the patience of 100 subjects (with no further supply of subjects in view) and seeing no means of controlling dominance with my present equipment, I have decided to present the material for what it may be worth and plan a more adequate attack later if possible. In general, I may say that, although certain of the results appear conclusive and definite when taken by themselves, there appear other results of such a nature as to support the suggestion that the number of subjects was not sufficient to rule out definitely the operation of chance factors.

The two stimuli used were shock in the left hand and shock in

[2]See bibliography of Wendt (5) or McGeoch (2).

the right hand. Each stimulus initiates a rather definite withdrawal which has many reflex properties. The two reflexes were found to be overtly specific. A shock in one hand before the training period resulted in the withdrawal of that hand only. The chief object in using these two stimuli was the possibility of controlling and varying the dominance factor. This would have been very difficult if I had used two qualitatively different stimuli. In this study it is assumed throughout that the withdrawal from a strong shock is a more dominant reflex than the withdrawal from a weak shock. This assumption must be kept in mind throughout. The results of the experiment can be applied to the concept of the conditioned reflex only in so far as differences in intensity do represent real differences in dominance. I do not know of a definition of dominance which would rule out such an assumption, nor do I think that a difference in modality is definitely implied in the concept of a difference in dominance. The withdrawal from a strong shock is considered to be more dominant than withdrawal from a weak shock in the sense that (*a*) if the two responses were mutually exclusive the former would prevail and (*b*) the former has more biological significance than the latter.

It may be that the concept of dominance has other vital attributes, and, in that case, those additional attributes would remain untested.

The arrangement of the experiment was suggested above and is given in more detail in the following chart:

	One shock stronger than the other		Both shocks of the same strength	
Shocks given simultaneously	I	L r	III	l r
One shock given before (¼") the other	II	l R	IV	r l

In this chart the Roman numerals refer to the different experimental groups. A strong shock is indicated by a capital letter and a weak shock by a lower-case letter. One letter directly above the other indicates simultaneous presentation. One letter preceding the other indicates that the former occurs one quarter of a second before the latter.

If a time interval is the sufficient and necessary cause of conditioning we should expect definite conditioning in II and IV and

none[3] in I and III. If, on the other hand, a difference in dominance is the essential thing we should expect definite conditioning in I and II but none[3] in III and IV. We shall speak of these and certain other possible facts as "the time-interval prediction" and "the dominance prediction," respectively, and also as "the prediction of the dominance hypothesis."

It will be noted that both hypotheses agree in predicting no conditioning at III and definite conditioning at II, but flatly controvert each other in the cases of I and IV. Experimental Groups II and III are valueless for the most definite comparison but are used for more detailed comparisons.

In discussing the direction of the conditioning the expression"L-R" means that a shock in the left hand comes to call forth a response from the right hand as well as (presumably) from the left. "R-L" means that a shock in the right hand induces a response in the left hand. No significance is attached to the fact that the letters are capitals.

The more detailed predictions of the two hypotheses may be indicated thus:

A. Predictions of the time-interval hypothesis. Either there shall be no conditioning or conditioning equally in both directions at I (*a*)[4] and at III (*b*), and if there is any there shall be just as much at I as at III (*c*) (the time order being the same for each). There shall be more R-L conditioning at IV than conditioning of either kind at III (*d*). There shall be more L-R conditioning at II than of either kind at I (*e*). There shall be just as much L-R conditioning at II as there is R-L at IV (*f*).

B. Predictions of the dominance hypotheses. Either there shall be no conditioning or conditioning equally in both directions at III (*a*) and at IV (*b*), and if there is any there shall be just as much at III as at IV (*c*) (the difference in dominance being the same). There shall be more R-L conditioning at I than of either type at III (*d*) and there shall be more L-R conditioning at II than of either type at IV (*e*). There shall be as much R-L conditioning at I as there is L-R at II (*f*).

The above predictions are condensed and tabulated in Table 3 of

[3]This "none" should be qualified by adding "or conditioning equally in either direction."

[4]Letters in parenthesis are to aid in the identification of the predictions.

the results and the actual result given. Out of a total of eight comparisons, there are two where the predictions agree and six where they are mutually exclusive.

Apparatus

The data included were obtained from two different experimental set-ups. The data for Experimental Groups II and IV *b* were obtained from the first set-up and the data for experimental Groups I, III, and IV*a* from the second. In the first set-up each hand was stimulated by the current from a separate inductorium. Thus all the switching had to be done through the primary windings. To try to secure equivalence, both primaries were supplied from the same source and served by the same interrupter. Owing to the fact that most of the shock came with the onset and cessation of the already interrupted current through the respective primaries, this device was unsatisfactory. In the case of Experimental Group II, however, where only a *difference* in intensity was demanded, it seemed to work well enough. Its unsatisfactory nature was discovered when I began to try to secure the same intensity in each hand. After complete records had been obtained from three subjects (Group IV*b*) the apparatus was changed so that only one inductorium was used and the switching was done between the secondary terminals and the two sets of electrodes.

The arrangement of the first set-up is indicated in Figure 1. The essential elements consist of two inductoriums (home-made from Ford spark coils) (*LI* and *RI*) supplied from the same lamp board and affected by the same interrupter (*Int.*). The left inductorium (*LI*) is permanently attached to the electrodes of the left hand (*LFE* and *LPE*) and the right inductorium (*RI*) is permanently attached to the electrodes of the right hand. The double-pole, double-throw switches *A*, *B*, and *C* are arranged so that the primaries of both *LI* and *RI* may be connected in series with pole *2* of the rotating switch *RS*. This switch (the commutator from a Model-T Ford car) rotates continuously, being driven by a small *D.-C.* motor at the rate of about 1 revolution per second. Thus the rotating arm comes in contact with *3* about ¼ second after its contact with *2*.

When both primaries are in series with pole *2*, the shock will occur simultaneously in both hands. The switches *A* and *B* can

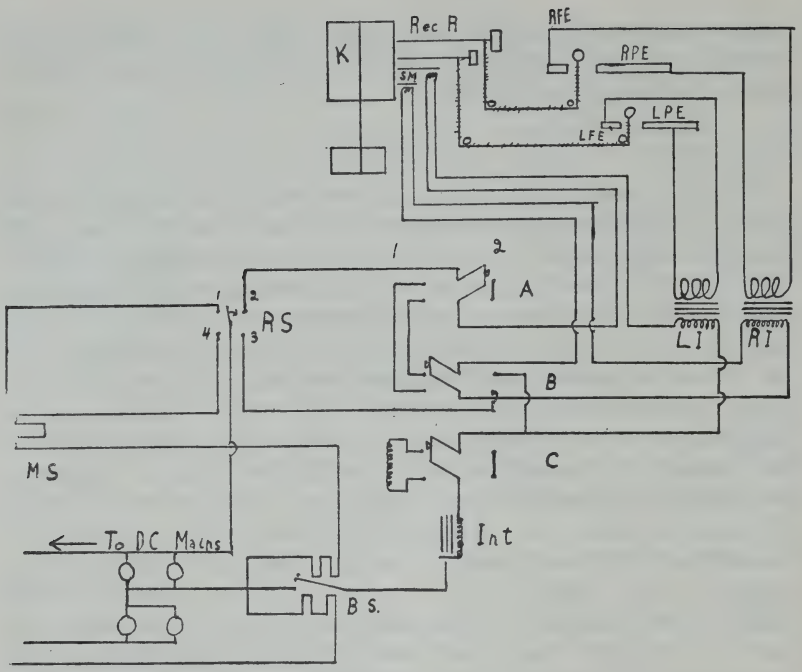

FIGURE 1

K—kymograph
Rec R—Recording reed
SM—signal magnets
RFE—right finger electrode
RPE—right palm electrode
LFE—left finger electrode
LPE—left palm electrode
RS—rotating switch with four contacts
LI—left inductorium
RI—right inductorium
MS—manual switch
BS—double action magnetic switch
Int—interrupter

also be arranged so that the primary of *LI* is attached to pole *2* and that of *RI* to pole *3*. In this way the shock in the right hand will occur ¼ second later than the shock in the left hand. The switch *C* is used to introduce a dummy primary having about the same resistance as the primary of either inductorium. When the switches are set for simultaneous occurrence of stimuli the current must pass

through both primaries in series. When, however, the switches are arranged for a sequence of shocks, the current would ordinarily pass through only one primary at a time. Switch *C* was used to insure that the current would always pass through two and only two primaries.

The other poles of the rotating switch were used to activate relays in such a way that, when a sequence of shocks was used, the shock produced by contact at *2* should precede that produced by contact at *3*. It will be seen that if the circuit through the rotating switch were closed when the arm was between *2* and *3* the primary in series with *3* would be activated first. In order to prevent this, two relays (*MS* and *BS*) were used. When it was decided to give a sequence of shocks, the switch at *MS* was closed manually. If the rotating arm were between *4* and *1*, the contact at *1* would complete a circuit around the lower magnets of *BS* and this would make possible the closing of the circuit through the interrupter and primaries, when the switch reached *2* and *3*, respectively. When the switch reached *4* it would complete a circuit through the magnets of *MS* and the upper magnets of *BS*, thus opening both circuits. If, however, the switch *MS* was manually closed while the rotating arm was between *1* and *4* (i.e., between *1* and *2*, *2* and *3*, or *3* and *4*) it (*MS*) would be thrown open as soon as the rotating arm reached *4* and the switch at *BS* would not be closed at all. By this arrangement the circuit through the primaries could only be closed when the rotating arm had passed *4* and had not yet reached *1*. Consequently, the primary in contact with *2* would always be activated first.

The onset of the shocks was recorded on a kymograph (*K*) by signal magnets (*SM*) in series with each of the primaries respectively. The response of the hand was recorded by means of a cord (casting line—shown by hatched lines in the figure) attached to each finger and from there run through a system of pulleys to a bamboo recording reed (*Rec R*). A fifth signal magnet (not shown in the figure) attached to a Cenco spark timer gave a time line in ¼ seconds.

As mentioned above, the apparatus proved very unsatisfactory for equating intensity. So long as only a definite difference in dominance was required, this set-up seemed adequate. But when I tried to secure the same intensity in each hand, empirical tests indicated a

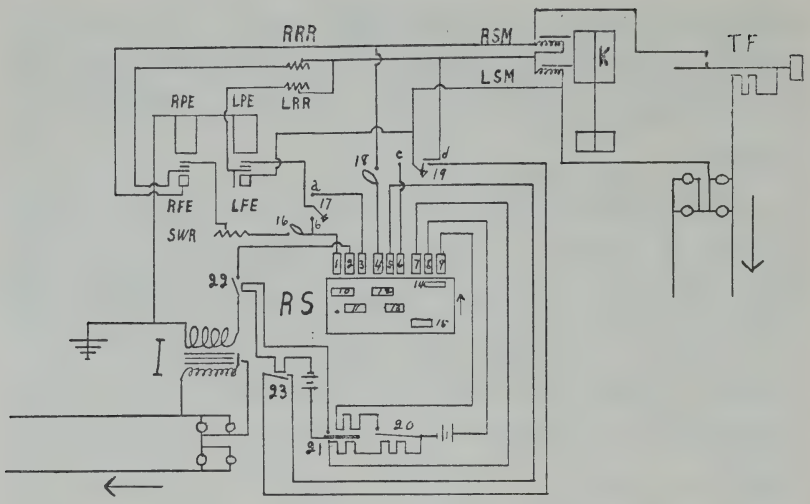

FIGURE 2

RRR—right recording resistance
LRR—left recording resistance
RSM—right signal magnet
LSM—left signal magnet
K—kymograph
TF—tuning fork (Cenco spark timer)
RPE—right palm electrode
RFE—right finger electrode
LPE—left palm electrode
LFE—left finger electrode
RS—rotating switch
I—inductorium

very considerable variation. This was due to differences in the inductoriums and possible differences in the make and break of the circuit at *1* and *2*.

In order to minimize these difficulties a second set-up illustrated in Figure 2 was used. In this only one inductorium was used and all the switching was done within the secondary circuit. Any variations of the resistance in the switching apparatus would have less influence on the high-voltage secondary current than on the low-voltage primary current because (*a*) the variations in resistance would be very slight in relation to the high initial resistance of the secondary and (*b*) the rapidity of the make and break of the secondary has no influence on the intensity of the shock, whereas the

rapidity of the make and break in the primary is an important factor in the intensity of the shock.

This switching through the secondary necessitated many modifications and invited others. The recording apparatus had to be changed, since the weak oscillating secondary current would not operate a signal magnet. To overcome this, the rotating switch (*RS*) was arranged so that several circuits could be operated independently of each other. This was accomplished by sinking copper strips (*10, 11, 12,* etc) into a wooden rolling-pin in such a way that they would come in contact with different pairs of copper tabs (*1, 2, 3,* etc.) as the rolling-pin rotated. The strips coming in contact with tabs *1, 2,* and *3* controlled the current through the electrodes, those coming in contact with *4, 5,* and *6* controlled the recording signals, and those coming in contact with *7, 8,* and *9* controlled the relays described in connection with the first set-up. These relays are arranged to operate switches *20* and *21,* the latter of which operates switches *22* and *23.* This complication was necessary because the gap across the contact points of relay *21* was not sufficient for the high-voltage current. Closing the manual switch *20* before strip No. *14* reaches the tabs *8* and *9* results in the closing of switches *22* and *23* as soon as *14* does reach *8* and *9* and in their remaining closed until *15* reaches *7* and *8.* Upon this latter contact a current is sent through the magnets of *20* and the lower magnets of *21,* ultimately breaking the circuit at *22* and *23.* With switch *22* closed the secondary current from the inductorium will pass from tab *2* to tab *1* as soon as *10* comes in contact with them. From tab *1* the current will pass to the single-pole, single-throw switch *16* and to pole *b* of the single-pole, double-throw switch *17.* If *16* is closed the current will pass through none or some of the slide-wire resistance (*SWR*) to the finger electrode of the right hand, through the hand to the palm electrode, and back to the inductorium. If *17* is at *b* the current will also go through the left hand. If, however, *16* is closed and *17* is at *a* the current will pass through the right hand when *10* reaches *1* and *2* and through the left hand when *11* reaches *2* and *3* (¼ second later).

The strips were so arranged that *10* reached *1* and *2* at the same time that *12* reached *4* and *5,* and that *13* reached *5* and *6* as soon as *11* reached *2* and *3.* By this means tabs *4, 5,* and *6* could be used to work recording devices. The method of recording used here

was that of short-circuiting the respective signal magnets [activitated by the tuning fork (*TF*)], thus reducing to almost zero the amplitude of the recording needle. Thus contact between *4* and *5* could be made to short-circuit the upper signal magnet *RSM* and contact between *5* and *6* would short-circuit *LSM,* or contact between *4* and *5* could be made by closing switch *18* and placing *19* at *d* to short both signal magnets. The arrangement at the *d* pole of *19* needs some explanation. If the switch is open or at *c* the two adjacent horizontal wires at *d* are in contact. Attached to the switch is a bakelite spur which pushes the upper wire away from the lower whenever *19* is placed at *d*.

In order to prevent any possibility of error, switches *16* and *18* were fastened together by a bakelite bar (i.e., really a double-pole, single-throw switch) so that if one were closed the other would be, too. Switches *17* and *19* were similarly joined together.

The response was recorded by breaking a partial short around the respective signal magnets. So long as the finger remained on the electrode a variable resistance (*RRR* or *LRR*) would be in parallel with the appropriate signal magnet, and the amplitude of the latter reduced (the reduction being controlled by the variable resistances); as soon as the finger was raised from the electrode (as in withdrawal from the shock) the shunt would be broken and all the current would go through the appropriate signal magnet and the full amplitude of the recording needle attained. Thus when the finger was on the electrode and no shock was being administered the record would show a row of limited vibrations. At the onset of the shock these would be reduced to practically zero and, with the withdrawal of the finger, would increase to the maximum amplitude. By this means all the data for one hand could be obtained from one needle (e.g., time line, stimulus, and response). Moreover, the imposition of the stimulus and response directly on the time line greatly facilitated the reading.[5]

The subject was placed in a cubicle made of beaverboard and a movable screen. In this cubicle were a chair and a table bearing the electrodes. On the walls were pasted pages of jokes from *Life, College Humor,* etc. Prominent in the midst of these was a problem

[5]The advantages to be gained from using variations in amplitude of the time line to record the response were first suggested to me by Mr. T. W. Ziegler.

in mental multiplication. The cubicle was open at the top and underneath the table. The noise was considerable and constant. It was produced by the motor, reducing gear, and rotating switch and the buzz of the inductorium. Frequent empirical tests were made to see if any secondary cues were present.

Procedure

Both in describing the experiment in pleas for subjects and in discussing it with individual subjects, the problem was spoken of as an attempt to see if the resistance of the skin to electricity was affected by mental work. The occurrence of the shocks was held to be an accident attendant on the measurement of that resistance. The occurrence of the shocks was ascribed to a sudden reduction of the resistance of the skin.

When the subject was seated this explanation of the experiment and the forthcoming shocks was repeated. He was told to let his fingers take care of themselves—to forget about them—neither to resist any tendency to move them nor to exaggerate it, but to attend to the multiplication. He was then shown how to place his hands and—in the first set-up—the cords were adjusted. His attention was called to the multiplication which he was told to begin at on a given signal. The screen was then placed and the recording needle adjusted, the subject being asked a number of times to lift each finger. The motor and tuning fork were then started and after a few seconds the first trials made. In these trials the inductorium was set at a minimum and the strength gradually increased until a response occurred. Each hand was tested separately until a consistent reaction was observed. The relative intensities and time orders were then set, the subject told to begin multiplying mentally, and the double stimulations begun. The usual procedure was to give from 7 to 10 of these and then test one hand alone, to give 4 or 5 more double stimulations and test the other hand alone. The double stimulations were given with random intervals ranging from 2 to 30 seconds between pairs. From 150 to 200 were given in the 30-minute period. The length of the experimental period was approximately 30 minutes. No subject was used for more than one period.

Frequent tests were made for conditioning to extraneous sounds such as the closing of the manual switch with no shock, or the

sudden movement of a chair, or a light tapping. Responses to such stimuli were recorded as "false conditioning." They occurred very seldom and only in the case of the last few subjects.

Subjects

The subjects were Summer School, College for Teachers, and A.B. students, both men and women.

Results

In the discussion of these results a "pair of shocks" or a "double stimulation" means that both hands were stimulated either simultaneously or ¼ second apart. A "trial" indicates the stimulation of one hand alone after a series of double stimulations have been given. Sometimes due to a failure of the apparatus an unintentional trial appears. There is no means of distinguishing between an intentional and an unintentional trial, nor is such a distinction desirable since the effect was the same. On the other hand, many attempts at trials may have failed to act as such. Those cases in which only one hand was stimulated and *neither* hand responded were not counted as trials—the assumption being that no stimulus was received.

Although ninety some subjects participated in the experiment, complete records were obtained from approximately fifty. The other subjects were dismissed early in the training period because of manifest inadequacies in the apparatus (during the preliminary period of testing set-ups) or suspected nervous condition of the subject (e.g., crying or jumping off the chair). Of the completed records all but 27 were discarded because of suspected inadequacies of the apparatus revealed after the test, or failure to secure any response, or a reaction-time in excess of ½ second (considered as indicating a purely "voluntary" reaction).

A summary of the results for each individual is given in Table 1. The results of the individuals of each experimental group are presented together. Preceding each group is given the arrangement of the shocks. In the second column of the table, *L-R* stands for trials for left-right conditioning (i.e., where the left hand alone is stimulated and the response of the right hand observed).

Each entry indicates a trial. The numerical part of each trial

TABLE 1
RESULTS OF INDIVIDUALS FROM EACH EXPERIMENTAL GROUP

Subject			No. of successess	Percentage of successes	Predominant* direction of conditioning
		Group I: Simultaneous, left stronger			
45	L-R	48x, 54x, 77x, 77x	0	0	About
	R-L	26x, 47x, 69c, 69x, 81x	1	25	even
46	L-R	38x, 74c	1	50	About
	R-L	49x, 61x, 69x, 76c	1	25	even
47	L-R	28x, 47x	0	0	About
	R-L	39x, 50x	0	0	even
49	L-R	32c, 70x, 87c?, 87x	1	50	About
	R-L	20x, 50x, 79c?, 87c, 87x, 90x	1	25	even
50	L-R	24c, FC† found and eliminated, 62c, 62x	1	100	About
	R-L	55c, 55x, 62x, 68c	1	67	even
		Group II: Left first and weaker			
12	L-R	26c, 31c, 42c, 53c, 53c, 53c, 53x, 53c, 53x	4	100	
	R-L	8x, 9x, 15x, 17x, 21x, 24x, 25x, 33x, 45x, 50x	0	0	L-R
13	L-R	8c, 8x, 27x, 27x, 27x, 27x, 27c, 48c, 52c, 53x, 57c, 64c, 64x, 64c, 64x, 64x	6	75	
	R-L	16x, 36x, 40x, 40x, 40x, 41c?, 45c, 60x, 60x, 71x, 71x, 71x	1	17	L-R
14	L-R	5c, 32x, 32c?, 47x, 53c, 64c, 64x	3	60	
	R-L	43c?, 47x, 47x, 59x, 59x, 66x	0	0	L-R
15	L-R	19x, 19x, 50x, 50x, 52x	0	0	About
	R-L	6c, 22x, 27x, 50x, 53x	1	20	even
17	L-R	19c, 30c, 33c, 43c, 45c, 45x, 45x, 45x, 45c, 47x	5	83	
	R-L	27x, 27x, 27x, 36x, 40x, 44x, 47x	0	0	L-R
18	L-R	50x, 56x, 71c	1	33	About
	R-L	59x, 69c, 82c	2	67	even
19	L-R	25x, 26x, 33x, 33c	1	25	About
	R-L	10x, 42x	0	0	even
		Group III: Simultaneous, same strength in each hand			
36	L-R	17c, 17c, 41x	2	50	
	R-L	29c, 29c, 29c, 29c, 29c, FC (eliminated), 29c, 29x, 41c	1	100	R-L
37	L-R	23c, 38c, 38x, 64x, 76c?, 76x	2	50	About
	R-L	27c, 42x, 60c, 69c, 69x	3	75	even
38	L-R	45c, 55c, 55x, 65c, 65x, 73x	3	75	
	R-L	31x, 41c, 60x, 70x	1	25	L-R

TABLE 1 (*continued*)

Subject			No. of successes	Percentage of successes	Predominant* direction of conditioning
39	L-R	10c, 33c, 33c, 33c	2	100	
	R-L	17x, 41x, 45x	0	0	L-R
40	L-R	9x, 17x, 32x, 53c, 71c, 71x	2	50	
	R-L	7x, 7x, 7x, 7x, 30x, 30x, 38x, 64x, 82x	0	0	L-R
		Group IVa: Right first and equal strength			
26	L-R	26c, 26x, 36c, 36c, 36c, 36c, 36c, 36c, 36c, 36x	2	100	About
	R-L	5x, 26c, 26x, 32c, 32c, 32c, 32c, 32c, 32c, 32c, 32c, 32c, 32x	2	67	even
33	L-R	13c, 13c, 13x, 21x, 24c, 24c, 24x, 37x	2	50	About
	R-L	19x, 21x, 32c, 32x, 36x, 36c	2	50	even
34	L-R	8x, 19c, 28x, 32x, 40x	1	20	About
	R-L	25c, 25x, 27x, 39x, 48c, 48x	2	50	even
35	L-R	16x, 27x, 37x, 38x	0	0	
	R-L	22c, 29x, 35c, 35c, 35c, 41c, 41c, 41c, 41x, 43x	3	60	R-L
42	L-R	10c, 10c, FC (elim.), 21c, 21c, 21c, 21x, 21x, 24x, 24x, 24x	1	50	
	R-L	16c, 21c, 21c, 21c, 21c, 21c, 21c, 21c, 21x, 24c, 24c, 24c, 24c, 24c, 24c, 24x	3	100	R-L
43	L-R	16x, 33c?, 33x, 44x, 45x	0	0	
	R-L	2c, 21c, 21c, 21c, 21c, 21x, 34c, 39c, 39c, 39c, 45c, 45c, 45c, 45c, FC (elim.)	5	100	R-L
44	L-R	12x, 29x, 32x, 35x, 40x, 44x	0	0	
	R-L	10c, 12x, 17c, 17x, 41c, 41c, 41c, 41c, 41x, 41x	3	75	R-L
		Group IVb: Left first and same‡ strength			
21	L-R	18x, 34c?, 35x	0	0	About
	R-L	13x, 21x, 28c, 32x, 32x, 32c, 35x	2	40	even
22	L-R	19c, 48c, 49c, 53x	3	75	About
	R-L	12c, 14c, 19x, 19x, 31c, 41c, 44c, 47c, 51x	6	75	even
23	L-R	19x, 19c, 61c, 66c	3	75	About
	R-L	9c, 24x, 31x, 41x, 65c, 67x, 67x, 69x, 69x	2	30	even

*A difference of 50% or more in successful trials is held to indicate greater conditioning in one direction than in the other.

†*FC* indicates conditioning to other things than shocks, such as sudden noises or clicks.

‡Under first set-up, equating of strength very rough.

indicates the number of double stimulations preceding it. The letter *c* indicates that the other hand responded.

For subject No. 45 the table should be read as follows: The first trial of the left hand alone was given after the subject had been given 48 combined stimuli. This trial was a failure. The next trial came after 54 combined stimuli, and was also a failure. The third trial was given after 77 combined stimuli, and the fourth was given immediately after the third (in the sense that no other type of stimulus intervened). The first two trials of the right hand alone were given after 26 and 47 combined stimuli, respectively, and were both failures. The next trial was given after 69 combined stimuli and was successful (i.e., a stimulation of the right hand alone elicited a response from the *left* hand as well as, presumably, from the right). The next trial occurred immediately afterward and was a failure. So also was the last trial after 81 combined stimuli.

The next column gives the absolute number of successes for each type of trial. No trials of *L-R* conditioning and one trial of *R-L* proved successful.

The next column gives the percentage of successful trials of each type. The percentage method of computation is far from satisfactory. Trials coming early in the series and trials immediately succeeding previous trials are by those very conditions less likely to show conditioning. The influence of the first factor (early appearance) is illustrated in the following hypothetical case:

		% L-R	% R-L
L-R	32*x*, 48*c*, 54*c*	67	
R-L	8*x*, 15*x*, 20*x*, 23*x*, 28*x*, 35*c*, 50*c*		30

Here the amount of conditioning is about the same in one case as in the other but the calculation is affected by the more frequent occurrence of early trials in the one case than in the other. No attempt has been made to correct for this error since some arbitrary standard of "earliness" would have to be adopted and such meddling would permit too many opportunities for the inevitable bias of the experimenter to enter.

The tendency of the percentage of successes to be reduced by the introduction of a series of unreinforced trials is obvious. To guard against that, it seemed best to count only the first of such a series of unreinforced trials as either successes or trials. If, however, we have first a failure and then, with no further reinforcement, a

success, the second item of the series is counted as a success and *both* items are counted as trials. In other words, if a series of unreinforced trials produced one or more successes, only one success is counted. If the appearance of that success is delayed, the delay counts against it by adding to the number of trials. Thus 62*c*, 62*x*, would be counted one trial, one success no failures, because the first trial would tend to produce a failure in the second trial (experimental extinction); 62*x*, 62*c* would be counted as two trials, one success, one failure; 62*c*, 62*c* would be counted one trial, one success, no failures; 62*x*, 62*x* would be counted one trial, one failure, no successes.

The last column attempts to sum up the results for each individual. As indicated in the note, the amount of conditioning is held to be about even unless there is a difference of 50% or more in the preceding two columns.

The last experimental group, 4*b*, needs some explanation. These subjects were tested with the old set-up and it was on the basis of their verbal reports (as well as of empirical tests by the experimenter) that the set-up was discarded because of the inability to secure even a rough approximation of equal intensity in each hand. The results are included because they are at variance with the other results and because that variance cannot be explained by the time-interval hypothesis. The defect of the condition was not one of time interval. The shocks were given definitely in left-right order. According to the time-interval hypothesis this (within limits) is all that is necessary to establish L-R conditioning. Since the time-interval hypothesis maintains that differences in intensity can be practically ignored, the failure of these results to satisfy that hypothesis cannot automatically be ascribed to the uncontrolled differences in intensity. The importance to be attached to this group will be taken up in the general discussion of the results.

The individual results in Table 1 have been summarized in Table 2. In this presentation several criteria are used to avoid the danger of bias inherent in any one criterion.

The first criterion used is that of the appearance of a conditioned reflex more than once. This indicates that the tendency was sufficiently strong to resist at least some experimental extinction. Headings 1 and 4 might profitably be combined since neither hypothesis discriminates between them. In columns 6 and 7 an at-

TABLE 2

THE DIFFERENT TYPES OF CONDITIONING (AS DETERMINED BY THREE DIFFERENT CRITERIA) OBSERVED IN THE RESPECTIVE EXPERIMENTAL GROUPS

Experimental Group ↓	Number of subjects showing definite* conditioning as follows:					The % of successes of each type from the trials of the subjects of column 1		The number and % (in parenthesis) of successes of each type from all subjects		Number of subjects with whom the conditioning seemed:			
	None	L-R only	R-L only	Both types	Total	L-R	R-L	L-R	R-L	Most L-R	About even	Most R-L	Total
No. of column →	1	2	3	4	5	6	7	8	9	10	11	12	13
No. I: Simultaneous, left stronger	4 Time	0	1 Dom.	0 Time	5	25	24	3 (30)	5 (27)	0	5 Time	0 Dom.	5
No. II: Left first and weaker	2	4 Time Dom.	1	0	7	12	14	25 (69)	4 (11)	4 Time Dom.	3	0	7
No. III: Simultaneous, equal strength	0 Time Dom.	3	0	2 Time Dom.	5			13 (62)	6 (35)	3	1 Time Dom.	1	5
No. IV*a*: Right first, equal strength	0 Dom.	0	5 Time	2 Dom.	7			7 (29)	20 (71)	0	3 Dom.	4 Time	7
No. IV*b*: Left first, equal strength (under old set-up; equality of strength questionable)	0 Dom.	0 Time	1	2 Dom.	3			6 (60)	10 (50)	0 Time	2 Dom.	1	3

*_Definite_ means that there were two occasions upon which the individual in question manifested the particular type of conditioned reflex. _Indefinite_ means its occurrence less than twice.

tempt is made to extract whatever evidence there may be from the *indefinite* cases. The next two columns indicate the total number of successes and (in parenthesis) the percentage of successes for each type of trial. The last four columns summarize the individual comments given in the last column of Table 1. The first line of Table 2 should be read thus: Of those subjects who received simultaneously a strong shock in the left hand and a weaker shock in the right hand, four failed to show two conditioned reflexes of either type (thus satisfying the time-interval hypothesis). No subject made two conditioned responses of the *L-R* type and less than two of the *R-L* type. One made two conditioned responses of the *R-L* type and less than two of the *L-R* type (thus satisfying the dominance hypothesis). No subject made two conditioned responses of both types. Of the four subjects who failed to make two conditioned responses of either type (i.e., the indefinite cases) 25% of the combined *L-R* trials were successful and 24% of the combined *R-L* trials were successful.

In columns 8 and 9 all the trials of a given type from all subjects in one group are lumped together and the number and the percentage (in parenthesis) of success of each type indicated. Thus for the first line we find conditioning in 30% of all the *L-R* trials and in 27% of all the *R-L* trials.

When the subjects are reclassified according to a less stringent criterion we find that no subject indicated a preponderance of *L-R* conditioning, that five seemed to show about as much conditioning of one type as of the other, and none exhibited a preponderance of *R-L* conditioning.

Throughout the table the word *"Time"* indicates where the time-interval hypothesis would predict the subjects to fall, and the word *"Dom."* indicates where the dominance hypothesis would predict the subjects to fall.

From Table 2 one can make a rough but perhaps very adequate comparison of the sufficiency of the two hypotheses. The hypotheses are in opposition only in Expermental Groups I and IV. In the case of Experimental Group I we find that, by the more rigid criterion, the time-interval hypothesis is satisfied by four cases, the dominance hypothesis by one. In the case of Experimental Group IV*a* we find, under the rigid criterion, that the time-interval hypothesis is satisfied by five cases, the dominance hypothesis by two, and under the less rigid criterion time interval is satisfied by four

and dominance by three. In IV*b*, under both criteria the time-interval hypothesis is satisfied not at all and the dominance hypothesis by two cases.

Both hypotheses are in agreement with regard to Experimental Groups II and III, but in III they are both curiously wide of the mark. This significant point will be discussed later.

We now move to the consideration of the results with a view to the more detailed predictions of each hypothesis. To do this we have compared each pair of adjacent groups in turn, stated the prediction of each hypothesis with regard to that comparison, and given the result in terms (*a*) of percentage of subjects satisfying a given criterion and (*b*) of percentage of successful trials (all trials of all subjects of the group being lumped) of a given type. This is given in condensed form in Table 3. A more extended statement of the predictions is given on page 114.

In Table 3 the hypotheses have been compared primarily at eight points and secondarily at three points. In items 2 and 3 the hypotheses agree in their predictions, both being satisfied in 2 and neither in 3. In the other six primary comparisons the respective predictions of the two hypotheses are mutually exclusive. Of these six comparisons the results of three definitely support the time-interval hypothesis, the results of the other three (Nos. 6, 7, and 8) are indefinite owing to the lack of a criterion upon the basis of which we can say there has been "the same" or "more" conditioning in one case than in another. A fair summary of these three indefinite comparisons would probably state that in no case is the dominance hypothesis satisfied and that the time hypothesis is satisfied in either comparison 7 or comparison 8. The time hypothesis predicts no difference at 7 and a definite difference at 8. The difference (in the third column) is identical in each comparison. If we decide that 70-60 represents equivalence, the time hypothesis is satisfied at 7. If 70-60 represents a real difference the time hypothesis is satisfied at 8. If we consider the comparisons in the fourth column there would be some basis for deciding that the time hypothesis was satisfied in *both* 7 and 8.

From the above we may say that in the six comparisons in which the predictions of the hypotheses disagree the dominance hypothesis is never satisfied, and the time hypothesis is satisfied certainly in four comparisons (numbers 1, 4, and 5 and *either* 7 or 8) and possibly in five comparisons (*both* 7 and 8).

TABLE 3

SUMMARY OF THE PREDICTED AND OBSERVED RESULTS FROM DIFFERENT EXPERIMENTAL GROUPS AND FROM COMPARISONS OF DIFFERENT PAIRS OF ADJACENT GROUPS

Item No.	Group or groups	Prediction of time-interval hypothesis	Prediction of dominance hypothesis	Result when the unit is one subject	Result when the unit is one trial	Hypothesis satisfied	
1	I	No uni-directional conditioning	Mostly R-L conditioning	20% R-L conditioning	3% less R-L conditioning	Time	
2	II	Mostly L-R	Mostly L-R	60% L-R	69% L-R	Both	
3	III	No uni-direct. cond.	No uni-direct. cond.	60% L-R	62% L-R	Neither	
4	IV*a*	Mostly R-L	No uni-direct. cond.	70% R-L	71% R-L	Time	
5	I and II	More L-R at II than of any type at I (*e*)*	No more L-R at II than R-L at I (*f*)	More L-R at II (60%) than R-L at I (20%)	More L-R at II (69%) than R-L at I (27%)	Time	
6	I and III	No more R-L at I than L-R at III (*c*)	More R-L at I than of any type at III (*d*)	Less R-L at I (20%) than L-R at III (60%)	Less R-L at I (27%) than L-R at III (62%)	Neither	
7	II and IV*a*	No more L-R at II than R-L at IV (*f*)	More L-R at II than R-L at IV (*e*)	Less L-R at II (60%) than R-L at IV (70%)	No more L-R at II (69%) than R-L at IV (71%)	Time	?
8	III and IV*a*	More R-L at IV than of either type at III (*d*)	No more R-L at IV than of either type at III (*c*)	More R-L at IV (70%) than L-R at III (60%)	More R-L at IV (71%) than L-R at III (62%)	Time	
9	IV*b*	Mostly L-R	No uni-direct. cond.	0% L-R	60% L-R	Dom.	
10	II and IV*b*	No more L-R at II than L-R at IV	More L-R at II than L-R at IV	More L-R at II (60%) than L-R at IV (0%)	More L-R at II (69%) than L-R at IV (60%)	Dom.	?
11	III and IV*b*	More L-R at IV than of either type at III	No more L-R at IV than of either type at III	Less L-R at IV (0%) than L-R at III (60%)	No more L-R at IV (60%) than L-R at III (62%)	Dom.	

*The *e* and other letters in parentheses refer to the predictions on page 114.

The above results are not substantiated by the secondary comparisons (9, 10, 11) using the three subjects of Experimental Group IV*b*. In the discussion of the apparatus I have mentioned that the data from these three subjects were obtained under such dubious conditions as to necessitate the rearrangement of the entire apparatus. Whether the negative results from IV*b* should offset the positive results from IV*a*, I do not know. This will be considered in the general discussion of the results.

General Discussion of Results

In this I shall attempt first to make a fair general statement of the outcome; secondly, to discuss especially those data which go contrary to the general trend; thirdly, to discuss hypotheses other than those of time interval and dominance which might explain the data; and, fourthly, to discuss the applicability of the results to the respective claims of the law of effect and of the conditioned reflex as the basic explanations of modification.

The results show that when there was no time interval between the stimuli but a definite difference of intensity practically no conditioning and certainly no definite "weak-strong" conditioning appeared. On the other hand, when there was no difference in intensity but a definite time interval between the stimuli, "first-second" conditioning did appear in five out of seven cases but failed to appear in three other cases tested under different conditions. Of the six occasions in which the predictions of the two hypotheses were mutually exclusive, the time-interval hypothesis was satisfied in four (and possibly in five) while the dominance hypothesis was satisfied in no case. If we take the three "irregular" subjects (Experimental Group IV*b*) mentioned above, by themselves, the dominance hypothesis would certainly be satisfied in one of the three comparisons and possibly in all three.

The significance of the contrasting results between Groups IV*a* and IV*b* is very difficult to determine. In general, I think the positive results of the seven subjects in IV*a* should overweigh the negative results of the three subjects of IV*b*. Many accidental disturbances might account for negative results, but could hardly account for positive uni-directional conditioning. Moreover, because of the greater number of cases in IV*a*, a combination of IV*a* and IV*b* would still indicate a predominance of uni-directional con-

ditioning. Finally, there is the fact that IV*b* represents only a portion of a group. The particular set-up was abandoned after testing the third subject because it was manifestly impossible to control the factor of dominance difference. As indicated above, this defect in the apparatus cannot automatically be used to account for the failure of the results to satisfy the time-interval hypothesis because the time-interval hypothesis claims to be able to ignore dominance difference within limits. The deficiency in the apparatus could be brought in only if the variation in intensity were such that these limits were exceeded. It is, of course, altogether likely that the limits were exceeded. The time-order hypothesis demands that the intensity of both stimuli be at least above the limen and that the intensity of the second be such as to call for an adjustment problem (i.e., be of such a nature that one response will combine with it to better biological advantage than will another response). If either of these conditions were not satisfied, the time-interval hypothesis could not be considered to have been tested. In actual fact it is not inconceivable that either stimulus might have frequently faded away to a subliminal strength.

The other disturbing feature is the rather definite uni-directional conditioning found when there was neither a time interval nor a difference in dominance. I have no suggestion to offer to account for this, nor do I know of any hypothesis which would explain conditioning under these circumstances. On the other hand, if this conditioning is not to be taken at its face value, such a caution should apply to all the other results.

The chief function of the incongruous results from Groups III and IV*b* is to indicate the necessity of a much large number of cases in order to determine whether or not the results from Groups I and IV*a* have real significance or are the result of chance fluctuation.

We come now to the discussion of other hypotheses which might account for the facts. The first such hypothesis would be that we merely have chance differences. This is supported by the small number of cases in each group, the limited number of trials for conditioning, and the incongruous results from Group III. Mere chance would account for the fact that we get no uni-directional conditioning in Group I and some in Group IV*a*. It could hardly account for almost all the other comparisons of groups (viz., I and II; I and III; II and IV; and III and IV) favoring the time hypothesis.

Another explanation would suggest that we are not dealing with true reflexes at all, and that, even if we are, we have not two distinct reflexes. During the course of the experiment I got the impression that when both *R-L* and *L-R* conditioning had been established, the extinction of the *R-L* type would lead to the extinction of the *L-R* type. Upon examining the records I could not find any evidence bearing on the possibility. It is certainly true that the response to a shock in the left hand is not confined to a withdrawal of the left hand.[6] It is, however, equally true that upon the first presentations such a shock did not involve a withdrawal of the right hand and in later presentations it often did. This represents a genuine case of the acquisition of a response. The organism is behaving differently now than he did before, and that difference must be ascribed to the treatment to which he was subjected.

Somewhat in line with this suggestion is the possibility that the conditioning in Group I was masked by the fact that a strong shock more than a weak shock would elicit the withdrawal of both hands. Thus if a strong shock, merely by virtue of being strong and irrespective of conditioning, were to result in the withdrawal of both hands, such a result would be recorded as a conditioned reflex and this would be counter to the dominance prediction. No such tendency appeared, however, before the individual had been subjected to the conditioning treatment.

Two factors lead to caution in the interpretation of the results: first, the limited number of cases, and, secondly, the incongruous results obtained from Groups III and IV*a*. The second of these factors might well be a function of the first. With these cautions in mind we may say that the evidence definitely favors the time-interval hypothesis. That is to say, the element of dominance is of little account in the establishment of the conditioned reflex. The reflex was established both with and without a difference of dominance so long as one stimulus preceded the other.

From this we may conclude that the conditions necessary for the establishment of the conditioned reflex are those postulated by the law of effect for any other type of learning. The conditioned re-

[6]This must not be mistaken for the phenomenon of generalization which deals with the similarity between two conditioned stimuli. In this study the similarity is between the conditioned and the unconditioned stimulus. See Anrep (1).

flex seems to be merely a case of trial-and-error learning. It involves all the attributes of the law of effect, namely, stimulus followed by a response, followed by a subsequent stimulus or an effect. As we have stated it above, the law of effect holds that if we repeatedly present S_1 and subsequently S_2, the response which S_1 will come to elicit consistently will be one which will combine with S_2 to produce a biologically valuable situation. S_2 determines which response will be attached to S_1. If there is only one response which, in combination with S_2, produces a valuable condition, that response is the only one which will consistently follow S_1.

Such is the law of effect. Now in the case of the conditioned reflex we have the added fact that the response which S_2 "imposes" on S_1 is the same response which S_2 itself originally elicits. This is natural when we realize that some situations are such that only one response will combine with them to produce a biologically valuable condition. It is not surprising that food, which itself evokes an approach response, should "insist" that an approach response be the only one to follow any stimulus habitually preceding it. That is to say, it will stamp out any connection which does not lead to approach. Thus the identity between the response originally possessed by the unconditioned stimulus and later acquired by the conditioned stimulus is merely an incidental thing, depending on the nature of the effect.

Summary

The principle of the conditioned reflex has been advanced as an explanation of learning preferable to the explanation by the law of effect. According to this principle, learning is due to the simultaneous occurrence of a dominant and a non-dominant reflex and that learning is not due to the backward operation of a subsequent stimulus or effect on a previously manifested bond.

The study was undertaken to examine the relative importance of dominance dfference *vs.* time interval (backward operation) in the establishment of the conditioned reflex itself. It was found that, when there is no difference in the intensity of two shocks but an interval of ¼ second between the application of one and the application of the other, conditioning resulted in five out of seven subjects, but that with a definite difference in the intensity of simultaneously presented shocks there was conditioning in only one subject out of five. These results were supported by other modes of comparison

but are to be regarded with caution because of the small number of subjects and because of the appearance of conditioning when there was neither a time interval nor a difference of intensity.

In so far as the results can be temporarily accepted, they indicate that the essential thing in the establishment of the conditioned reflex itself is the *back action* of an effect.

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LE RÉFLEXE CONDITIONNEL COMME L'EXPLICATION DE LA FORMATION DES HABITUDES: I. LES FACTEURS ESSENTIELS DANS L'ÉTABLISSEMENT DU RÉFLEXE CONDITIONNEL

(Résumé)

Beaucoup d'auteurs ont maintenu que, comme explication de l'apprentissage, le principe du réflexe conditionnel est préférable à la loi de l'effet. Le réflexe maintient que l'apprentissage est due à l'apparence simultanée d'un réflexe dominant et non dominant et non à l'opération arriérée d'un stimulus subséquent ou effet sur un lien antérieurement manifesté.

Cette étude a eu comme but l'importance relative d'une différence d'intensité (dominance) et de l'intervalle de temps (opération arriérée) dans l'établissement du réflexe conditionnel lui-même. On a trouvé que quand deux chocs électriques de même intensité sont appliqués à un intervalle de 4 secondes entre eux, le conditionnement s'est montré chez 5 sur 7 sujets, mais qu'avec une différence définie dans l'intensité des chocs simultanément présentés il s'est montré un conditionnement seulement chez un sujet sur 5. Ces résultats ont été soutenus par d'autres moyens de comparaisons mais on doit les considérer avec précaution à cause du petit nombre de sujets et à cause de l'apparence du conditionnement quand il n'y a eu ni un intervalle de temps ni une différence d'intensité.

Ces résultats expérimentaux indiquent que la chose essentielle dans l'établissement du réflexe conditionnel est l'*action en arrière* d'un effet.

Stephens

DER BEDINGTE REFLEX ALS ERKLÄRUNG DER GEWOHNHEITS-BILDUNGEN: 1. DIE WESENTLICHEN FAKTOREN IN DER ERSTELLUNG BEDINGTER REFLEXE

(Referat)

Viele Autoren haben sich damit zufrieden gegeben, dass als Erklärung des Lernens das Prinzip des bedingten Reflexes dem Gesetz der Wirkung vorzuziehen sei. Der bedingte Reflex verficht, dass das Lernen auf simultanem Geschehen eines dominanten und eines nichtdominanten Reflexes und nicht auf der Rückwirkung eines nachfolgenden Reizes oder einer Wirkung auf eine vorher manifestierte Verbindung (bond) beruhe.

Diese Untersuchung versuchte die relative Wichtigkeit des Stärkeunterschiedes (Vorherrschen, dominance) verglichen mit dem Zeitintervall (Rückwirkung, backward operation) in der Erstellung eines bedingten Reflexes selbst zu prüfen. Man fand, dass, wenn zwei gleich starke elektrische Schläge mit vier Sekunden Abstand verabreicht werden, die Erstellung bedingter Reflexe bei 5 aus 7 Versuchspersonen stattfand, aber dass bei einem bestimmten Unterschied in der Stärke der gleichzeitig verabreichten Chocs sich die Erstellung nur bei einer von 5 Versuchspersonen einstellte. Diese Ergebnisse wurden durch andere Vergleichsverfahren unterstützt, müssen aber mit Vorsicht betrachtet werden, wegen der kleinen Zahl von Versuchspersonen und weil die Erstellung bedingter Reflexe auftritt, auch wenn kein Zeitintervall, noch ein Stärkeunterschied vorhanden war.

Diese probeweisen Ergebnisse zeigen, dass der wesentliche Faktor in der Erstellung des bedingten Reflexes die *Rückwirkung* eines Effektes ist.

STEPHENS

THE INFLUENCE OF MUSIC ON MEMORY*

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PAUL L. WHITELY

INTRODUCTION

That music has an influence upon human responses is a fact that has been recognized from the earliest periods of recorded history. As a problem for experimental investigation, the influence of music on behavior has only fairly recently been subjected to experimental scrutiny. Two rather recent books (1, 3) give reports of experimental investigations in this field. These books suggest clearly that a vast field is opened up for experimental investigation, and that "the province of music and its effects is profitable and fascinating subject-matter for the research of the scientist, as well as for the intuition of the artist or the meditation of the philosopher."

Most of the experimental studies have dealt with the influence of music upon the simpler processes, mainly motor processes, individual differences in musical appreciation, the nature and sources of musical enjoyment, the affective aspects of music, as well as the various bodily effects. There has been little attempt to investigate the influence of music upon the so-called "higher mental processes."

In this paper are reported the results of four different experiments, designed to test the influence of music upon the memorial process. In the first experiment, the music served as a prior condition, whereas in the other three experiments the music served as context during the process of learning. Since the details relating to the procedure of the experiments varied in each case, they are reported in connection with the discussion of each experiment separately.

FIRST EXPERIMENT: LEARNING OF SORROW AND JOY WORDS WITH MUSIC AS PRIOR CONDITION

The learning materials employed in this experiment consisted of two types of words, the one connotative of sorrow or suffering,

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while the other was denotative of joy or exhilaration. Three lists, each consisting of 18 words, were prepared, the words of each list being selected from a much larger number of words, which had been judged by a committee of 12 people as being most connotative of sorrow or of joy. The words were selected so that they were all of about the same degree of familiarity, and none was employed which is not in common usage. The words were carefully arranged so that those which are obviously associated did not occur together in the series. Words that rhymed or began with the same letter did not occur together.

Each list was typewritten on a sheet of paper, and was placed face down before the subject. When the signal was given the subjects turned the sheets over and memorized as many of the words as possible during the two minutes allowed for learning.

The material was learned under three conditions: (1) when no controlled activity preceded learning, which we have designated as the normal condition; (2) when the learning of the words was preceded by the playing of a Victor record, called "Lilac March," of sprightly rhythm; and (3) when "Ase's Death," from the "Peer Gynt Suite," a slow funeral march, preceded the learning activity. Each record occupied four minutes' duration, and the subjects were requested to listen attentively to the music, endeavoring to enter into its spirit, immediately following which the words were memorized. The ostensible purpose of the music was to arouse or create a definite set or mood prior to the learning activity.

The subjects, students of general psychology and the psychology of learning, were divided into three groups. One group consisted of 12 subjects; whereas each of the other two groups was composed of 15 subjects. Each group learned each of the three sorrow lists and each of the three joy lists under the three conditions of learning in a counterbalanced practice order.

The results of this experiment are summarized in Table 1, which

TABLE 1
MEANS AND SIGMAS FOR THE LEARNING OF SORROW AND JOY WORDS

	Sorrow words			Joy words		
	Normal	"Lilac March"	"Ase's Death"	Normal	"Lilac March"	"Ase's Death"
Av. no. words	11.03	11.00	10.87	10.87	10.71	10.31
$\sigma_{dist.}$	2.46	2.29	2.52	2.45	2.51	2.74
σ_M	.39	.36	.40	.39	.39	.44

gives the average number of sorrow and joy words memorized for the three conditions of learning, the sigmas of the distributions, and the sigmas of the means. The average number of sorrow words learned for the normal, "Lilac March," and "Ase's Death" conditions were 11.03, 11.00, and 10.87, respectively; while the average number of joy words memorized for the same three conditions were 10.87, 10.71, and 10.31, respectively. Obviously, the differences between the various conditions are small, but the conditions show the same consistency for the two kinds of words. Also, it will be observed that as the third condition ("Ase's Death") shows the least returns for learning, the subjects also exhibit greatest variability under this condition of memorizing.

The reliability of the data is seen in Table 2, which shows the various comparisons, the differences of the means, the sigmas of the differences, the ratio of the differences of the means to the sigmas of the differences, and the chances in 100 that the true differences are

TABLE 2
SHOWING THE RELIABILITY OF THE DATA IN TABLE 1

Words	Comparisons	Diff. of M	$\sigma_{diff.}$	Diff./$\sigma_{diff.}$	Chances in 100
Sorrow	Normal-"Lilac March"	.03	.53	.06	51
	Normal-"Ase's Death"	.16	.56	.29	62
	"Ase's Death"-"Lilac March"	.13	.54	.24	60
Joy	Normal-"Lilac March"	.16	.55	.29	62
	Normal-"Ase's Death	.56	.59	.95	83
	"Ase's Death"-"Lilac March"	.40	.59	.68	75

greater than zero. The sigma of the differences is, in each instance, greater than the difference of the mean, and therefore the differences are not deemed reliable in accordance with accepted statistical criteria. The reliability of the differences for the joy words is greater than that for the sorrow words.

SECOND EXPERIMENT: LEARNING OF MONOSYLLABIC WORDS WITH MUSIC AS CONTEXT FOR MEMORIZING

The procedure and results of this experiment may be summarized very briefly. The learning material for this experiment consisted of three- and four-letter monosyllabic words. Each list of words to be memorized consisted of 18 words, and the number

of three-letter and four-letter words was the same for each list. Each list was typewritten on a sheet of paper and placed face down before the subject. The lists were learned under the three conditions described in the previous experiment, which description need not be repeated.

The subjects for this experiment were students of general psychology. Prior to the experiment the 128 subjects participating in the experiment were divided into six groups, and the groups learned the lists, under the conditions heretofore described, in a counterbalanced practice order. Differing from the previous experiment, the music served as context and was played during the process of learning.

TABLE 3

MEANS AND SIGMAS FOR THE LEARNING OF MONOSYLLABIC WORDS

	Normal	"Lilac March"	"Ase's Death"
No. measures	113	110	114
Av. no. words	13.50	12.62	12.81
Av. no. errors	.15	.29	.12
$\sigma_{dist.}$	2.68	2.97	3.11
σ_M	.25	.28	.29

The data for this experiment are shown in Table 3, which indicates the number of measures for each condition, the average number of words memorized, the average number of errors, the sigmas of the distributions, and the sigmas of the means, for the three conditions of learning. The average number of words for the three conditions of learning, in the order presented in the table, were 13.50, 12.62, and 12.81, respectively. Again, we see that the normal condition is the most favorable condition for learning. Not only are more words learned under the normal condition, but also it will be observed that the variability of the scores is less for this condition.

The number of errors was relatively small for the three conditions of learning, although it is interesting to observe that about twice as many errors were made when fast music was played as were made under the other two conditions.

Table 4 presents the reliability of the data shown in Table 3. The columns are the same as those described in Table 2. It will be immediately observed that the differences between the first and second and the first and third conditions give a fairly high degree

TABLE 4
RELIABILITY OF THE SCORES SHOWN IN TABLE 3

Comparisons	Diff. of M	$\sigma_{diff.}$	Diff./$\sigma_{diff.}$	Chances in 100
Normal-"Lilac March"	.88	.37	2.38	99
Normal-"Ase's Death"	.69	.38	1.82	96
"Lilac March"-"Ase's Death"	.19	.40	.47	68

of reliability, while the difference between the second and third conditions gives a fairly low statistical reliability.

THIRD EXPERIMENT: LEARNING, RECALL, AND RELEARNING BY THE ANTICIPATION METHOD WITH MUSIC AS CONTEXT

In this experiment, the purpose was to determine the influence of music upon learning, as well as upon recall and upon relearning, when the learning was accomplished by the anticipation method. The learning materials employed in this experiment were lists of three- and four-letter monosyllabic words, as described in the second experiment. The words were either nouns or verbs, and many of the words could be construed as either nouns or verbs.

The words were serially exposed by means of a hand-operated memory drum. A period of two seconds was given for each word exposure in obtaining the learning scores, the recall scores, and the relearning scores. The writer, who served as experimenter, practiced the operation of the drum until its operation became more or less automatic. A stop-watch, placed on the table, served as a check in keeping constant the time of exposure for each word.

The anticipation method was employed in learning the material, as well as in recall and in relearning. The list of words was presented once, and thereafter the subject attempted to anticipate each succeeding word just before its presentation behind the aperture of the drum. It was assumed that the list was learned when the subject was able to anticipate successfully each succeeding word before it was presented. Thus, the number of presentations for learning was taken to mean the total number of times each list was presented. A subject would learn list one, for example, and then recall and relearn the list 15 minutes later. Between learning and recall, all subjects engaged in a common activity, namely, that of underscoring two-, three-, four-, five-, or six-letter words of pied

type, depending upon the day. Each subject was given a preliminary practice list in order that he might acquaint himself with the general method and technique of learning.

Each of the 25 subjects learned, recalled, and relearned five lists, each consisting of 18 words, under each of five different conditions: (1) under the normal condition, when there was no controlled activity; (2) while "Ase's Death," from the "Peer Gynt Suite," was played; (3) while "Nights of Gladness" waltz was played; (4) while there was played the "Dance of the Wooden Soldiers"; and (5) while a song in French, "Le Mal du Pays," was played and sung. "Ase's Death," as is well known, is a slow funeral march, and plaintive in mood. The selection, "Nights of Gladness," is a sprightly waltz, with a smooth rhythm. The "Dance of the Wooden Soldiers" is a fast fox-trot. The song in French was sung by a soprano, and is fairly slow in tempo. Each selection was played on an Edison Laboratory model phonograph.

TABLE 5
ILLUSTRATIVE CYCLE

Subject	Days 1	2	3	4	5
A	Normal—1	"Ase's"—2	Waltz—3	Dance—4	Song—5
B	"Ase's"—2	Waltz—3	Dance—4	Song—5	Normal—1
C	Waltz—3	Dance—4	Song—5	Normal—1	"Ase's"—2
D	Dance—4	Song—5	Normal—1	"Ase's"—2	Waltz—3
E	Song—5	Normal—1	"Ase's"—2	Waltz—3	Dance—4

Table 5 represents a sample cycle, which is inserted at this point to illustrate more clearly the order of procedure. Thus, on the first day, Subject A learned, recalled, and relearned list 1 under the normal condition; on the second day, he learned list 2 while "Ase's Death" was being played, etc. Subject B learned, recalled, and relearned list 2, on the first day, while "Ase's Death" was played; on the second day, this subject learned list 3 while the waltz was being played, etc., etc. Thus it may be seen, from the illustrative cycle, that each list was learned, recalled, and relearned by each individual in a counterbalanced practice order. Or, in other words, each list and condition of learning appear in all serial positions, thus eliminating the possibility of practice effects or differences of difficulty of the various lists favoring any one condition.

There were five different cycles in all, thus providing 25 measures

each for learning, recall, and relearning, for the five experimental conditions.

The results of this experiment are shown in Table 6, which

TABLE 6
MEANS AND SIGMAS OF LEARNING, RECALL, AND RELEARNING BY ANTICIPATION METHOD

	Normal	"Ase's Death"	Waltz	Dance	Song
		Learning			
Av. no. rep.	15.72	13.96	15.56	14.96	16.00
$\sigma_{dist.}$	5.84	5.04	4.62	4.74	5.24
σ_M	1.17	1.01	.92	.95	1.05
		Recall			
Av. no. words	15.36	15.40	15.00	14.88	15.44
$\sigma_{dist.}$	2.44	2.59	2.03	2.34	2.51
σ_M	.49	.52	.41	.47	.50
		Relearning			
Av. no. rep.	3.32	3.20	3.56	3.40	3.36
$\sigma_{dist.}$	1.73	1.88	1.86	2.24	1.62
σ_M	.35	.38	.37	.55	.32
Saving score	78.9	77.9	77.2	77.3	79.0

presents the means, the sigmas of the distributions, and the sigmas of the means, of the learning, recall, and relearning scores for the five experimental conditions of learning. We shall discuss in turn the learning, recall, and relearning scores.

Learning. The mean number of repetitions for the five conditions of learning are as follows: normal, 15.72; "Ase's Death," 13.96; waltz, 15.56; dance, 14.96; and song, 16.00. It will be observed that the fewest number of repetitions were required under the second condition, "Ase's Death," while the poorest condition was that in which the song was heard. The next to the poorest condition is the normal condition, and it will be noted further that the variability of the scores is slightly greater for the normal and song conditions. Least variability of scores is shown for the waltz condition.

Recall. It will be observed that differences between the various recall scores are very slight. It is not clear that the recall scores bear any consistent relationship to the learning scores. Whereas, in terms of absolute scores, the song condition is the poorest condi-

tion for learning, it is the best condition for recall. One explanation of this fact would be suggested by the consideration that, since greater effort was expended in learning under this condition, greater returns are obtained in recall. The "Ase's Death" condition is the best condition for learning and the second best condition for recall. This fact, however, appears inconsistent with the explanation offered above with respect to the difference in the learning and recall scores for the song condition, since the difference in the mean number of repetitions for learning between the "Ase's Death" and song conditions is 2.04 repetitions. This observation would lend support to the view that there is an actual differential effect produced by the music, irrespective of the number of repetitions for memorization.

Relearning. Again, the differences between the various relearning scores are very small. It is interesting to observe, however, that the condition, "Ase's Death," which was the best for learning, is also the best condition for relearning. Furthermore, it is interesting to note that retention, when measured in terms of recall and relearning, gives different results. This fact has been noted in other experiments, and the results here presented may be merely another instance of the principle that retention is a function of the method of measurement used. A slightly different relationship obtains when the savings scores are computed. In terms of saving, the song condition becomes

TABLE 7
CONSISTENCY OF THE VARIOUS CYCLES

	Normal	"Ase's Death"	Waltz	Dance	Song
			Learning		
1st cycle	17.0(4)	16.2(1½)	17.6(5)	16.8(3)	16.2(1½)
2nd cycle	16.6(5)	14.6(2)	15.6(4)	13.8(1)	15.4(3)
3rd cycle	15.2(4)	12.4(1)	14.2(3)	13.2(2)	17.2(5)
4th cycle	16.8(5)	14.2(1)	15.4(3)	14.8(2)	16.2(4)
5th cycle	13.0(4)	12.4(1)	15.0(2½)	16.2(5)	15.0(2½)
			Recall		
1st cycle	16.4(1)	15.2(3)	15.6(2)	13.6(5)	14.6(4)
2nd cycle	15.8(2)	15.2(3)	14.6(4)	13.5(5)	16.8(1)
3rd cycle	15.6(4)	16.0(3)	14.6(5)	16.4(1½)	16.4(1½)
4th cycle	14.8(3½)	15.8(1)	14.8(3½)	15.6(2)	13.2(5)
5th cycle	14.2(5)	14.8(4)	15.4(2½)	15.4(2½)	16.4(1)
			Relearning		
1st cycle	4.0(2)	3.8(1)	4.4(3)	4.8(5)	4.6(4)
2nd cycle	2.6(1½)	2.8(3)	2.6(1½)	3.8(5)	3.0(4)
3rd cycle	2.8(3)	2.0(1)	3.8(4½)	3.8(4½)	2.4(2)
4th cycle	3.6(4)	3.4(2½)	3.4(2½)	2.2(1)	4.4(5)
5th cycle	3.6(3½)	4.0(5)	3.6(3½)	2.4(1½)	2.4(1½)

the best condition, the normal still holding second position, and "Ase's Death," dance, and waltz, in order. Admittedly, the differences with which we are dealing are small differences. However, it is an interesting fact that the song appears to be the poorest condition for learning, but the best condition for retention, when measured in terms of recall and of saving. If this fact obtains, it may be stated that, while the music (song) serves as a slight distraction in learning, its presence as context into which the material is assimilated serves to give the subjects something of a better pattern for retention.

The consistency of the five cycles is shown in Table 7. The table

TABLE 8
SHOWING THE RELIABILITY OF THE DATA IN TABLE 7

Comparisons	Diff. of M	$\sigma_{diff.}$	Diff./$\sigma_{diff.}$	Chances in 100
	Learning			
Normal—"Ase's Death"	1.76	1.54	1.14	87
Normal—Waltz	.16	1.79	.09	54
Normal—Dance	.76	1.81	.42	66
Normal—Song	.28	1.57	.17	57
"Ase's Death"—Waltz	1.60	1.36	1.17	87
"Ase's Death"—Dance	1.00	1.38	.72	76
"Ase's Death"—Song	2.04	1.45	1.40	92
Waltz—Dance	.60	1.32	.45	67
Waltz—Song	.44	1.39	.31	62
Dance—Song	1.04	1.41	.73	77
	Recall			
Normal—"Ase's Death"	.04	.71	.05	52
Normal—Waltz	.36	.64	.56	71
Normal—Dance	.48	.68	.69	76
Normal—Song	.08	.70	.11	54
"Ase's Death"—Waltz	.40	.66	.61	73
"Ase's Death"—Dance	.52	.70	.70	76
"Ase's Death"—Song	.04	.72	.05	52
Waltz—Dance	.12	.62	.19	58
Waltz—Song	.44	.64	.69	76
Dance—Song	.56	.69	.81	79
	Relearning			
Normal—"Ase's Death"	.12	.51	.23	60
Normal—Waltz	.24	.51	.47	67
Normal—Dance	.08	.65	.12	54
Normal—Song	.04	.47	.09	54
"Ase's Death"—Waltz	.36	.53	.68	75
"Ase's Death"—Dance	.20	.67	.29	62
"Ase's Death"—Song	.16	.49	.32	63
Waltz—Dance	.16	.66	.24	60
Waltz—Song	.20	.48	.42	65
Dance—Song	.04	.64	.06	52

shows the mean scores and the relative positions of the various conditions in the successive cycles for learning, recall, and relearning. The consistency, on the whole, is not very marked. However, it may be stated that, for learning, the "Ase's Death" condition is fairly consistently the best condition, while the normal condition is fairly consistently the poorest condition.

The reliability of the data is to be seen in Table 8. It will be observed that none of the differences satisfies the condition of perfect reliability, where the difference between the means is three times its sigma. However, five of the differences of learning approximate a fairly high degree of reliability, the differences between the "Ase's Death" and the song conditions being most reliable. On the whole, it appears that the differences of the learning scores are more reliable than those of recall and relearning.

Fourth Experiment: Influence of Music upon the Learning of Poetry

This experiment was designed to test the influence of two different kinds of music upon the acquisition of poetry. The poetry, or learning material, consisted of four stanzas of "Gilpin's Ride."[1] The type of poem may be readily discerned by quoting the first stanza. There were three other stanzas similar in meter and rhythm to this one:

> Now Mistress Gilpin, careful soul,
> Had two stone bottles found,
> To hold the liquor that she loved,
> And keep it safe and sound.

Since each subject participating in the experiment memorized under three different experimental conditions, there were three sets of four stanzas each.

The learning material was typed on letter-size sheets of paper, and was placed face down before the subjects. Each of the 30 subjects memorized under three different conditions, namely, (1) under the normal condition; (2) while the "Largo," being Part III of "The New World Symphony," was played; and (3) while "The Storm" from "William Tell" was played. The first type of music is rather slow, majestic, and melodious in character, while the sec-

[1]To be found in "Notes for Instructors" to accompany *Experiments in Psychology* by Foster and Tinker (2, pp. 22-24).

ond, "The Storm," may be described as tempestuous. Four minutes were given for each condition of learning. The order of learning was counterbalanced so as to eliminate the possibility of practice effects or differences of difficulty of the various sets favoring any one condition.

The learning scores were determined by ascertaining the average number of lines retained upon immediate recall, the recalls being obtained by having each subject write on a blank sheet as many of the lines as he could remember at the expiration of the four-minute period for learning. Also, account was taken of four different kinds of errors, namely, omissions, misplacements, additions, and substitutions.

Table 9 shows the mean number of lines memorized for the three conditions of learning, as well as the sigmas of the distributions and the sigmas of the means. The mean number of lines memorized for

TABLE 9
MEANS AND SIGMAS FOR LEARNING OF POETRY

	Normal	"Largo"	"The Storm"
Mean	9.38	8.25	7.15
$\sigma_{dist.}$	3.51	3.62	2.70
σ_M	.64	.66	.49

the three experimental conditions is as follows: normal, 9.38; "Largo," 8.25; and "The Storm," 7.15. It is readily observed that the normal condition is the best condition of learning, "The Storm" the poorest condition, with the "Largo" occupying an intermediate position between the two. Thus it would appear that the musical context during a process of memorizing poetry serves as a slight distraction.

That the differences between the means exhibit a relatively high degree of reliability is observed by an examination of Table 10.

TABLE 10
RELIABILITY OF DATA IN TABLE 9

Comparisons	Diff. of M	$\sigma_{diff.}$	Diff./$\sigma_{diff.}$	Chances in 100
Normal—"Largo"	1.03	.92	1.12	86
Normal—"The Storm"	2.23	.81	2.75	100
"Largo"—"The Storm"	1.10	.82	1.34	91

Especially reliable is the difference between the normal and "The Storm" conditions, since the chances are 100 out of 100 that the true difference is greater than zero.

As has been indicated, account was taken of four different kinds of errors: omissions, misplacements, additions, and substitutions. The number of errors for each condition is relatively small. Due to the fact that the error scores do not represent a normal distribution, coupled with the fact that the differences between the means do not represent significant differences, a table presenting error scores is omitted.

Discussion of Results

Save for the third experiment, the results of the various experiments are consistent in the sense that the normal condition is the best condition for memorizing. It is somewhat hazardous to state just what effects the music had upon the subjects, since there was no attempt to record bodily changes graphically or to obtain protocols from the subjects with regard to the possible influences, introspectively determined. An examination of the literature bearing on the subject reveals the fact that, in general, music appears to have a dynamogenic effect upon work involving the grosser musculature, although diminutions of activity are reported when certain melodies are employed. Music of a sad character and of slow rhythm caused a decrease in the capacity for muscular work. Again, it is reported that instrumental music is preferable to vocal, and that familiar music is more desirable than unfamiliar. Some artists like to paint to music, stating that it enhances processes of imagery. But not only is there an influence upon imagery processes, there is also an effect produced upon the strokes with the brushes, due in all probability to a reinforcement of reflexes.

So far as our results are concerned, the only suggestion of a facilitating or dynamogenic effect is seen in Experiment III. In this experiment, where the anticipation method of learning was employed, the learning and also the relearning are slightly superior under the condition of slow music, or "Ase's Death." In the main, from our results it appears that the influence of music upon a process of memorization is of a decremental nature. Moreover, we conclude that different kinds of music produce differential effects upon memorizing. For example, in the fourth experiment, music of

the tempestuous sort, with large and marked differences in tempo, rhythm, and intensities, and with a wide pitch range and striking dynamic effects, is much more distracting in its influence than the smoother music represented by Dvorak's "Largo," where the changes in rhythm, harmony, and intensity are more carefully graded. Neither condition, however, is as good as the normal condition.

Summary and Conclusions

This paper presents the results of four different experiments concerning the influence of music upon memorization. In the first experiment, the subjects participating in the experiment learned joy and sorrow words with music as a prior condition. Monosyllabic words were learned in the second experiment with music as context during memorization. In the third experiment, monosyllabic words were learned, and, after an interval of 15 minutes, recalled and relearned by the anticipation method. Four different types of music were employed in this experiment. Lastly, the influence of two different types of music upon the memorization of poetry was tested. The following conclusions emerge on the basis of the objective data:

1. When music serves as a prior condition to the learning of joy and sorrow words, the results indicate that the musical conditions, both slow music ("Ase's Death") and fast music ("Lilac March") exert a slightly detrimental influence upon learning, as compared with the normal condition. However, the differences between the control and experimental conditions are very slight and possess low statistical reliability. Albeit, the learning scores for the two kinds of words show the same order of superiority for the three conditions of learning.

2. Likewise, in the learning of monosyllabic words, the normal condition shows a slight superiority over both of the musical conditions mentioned above.

3. When memorization is accomplished by the anticipation method, fewest trials are taken under the condition of slow music ("Ase's Death"), whereas the greatest number of trials is required under the song condition. The normal condition is next to the poorest condition. The recall scores, after a period of 15 minutes, do not reveal any very significant differences, although, in terms of absolute scores, the song condition, which is the poorest for learning, is the best for retention when measured in terms of recall scores.

The differences in the relearning scores for the various conditions are also slight. We have noted, however, that, as the "Ase's Death" condition is the best for learning, it is also the best condition for relearning. Moreover, we have noted the fact that retention, when measured in terms of recall and relearning, gives variable results.

4. In the memorization of poetry, the results indicate clearly the superiority of the normal condition over the two musical conditions, "Largo" and "The Storm." Least returns are obtained when music of a tempestuous character serves as context.

5. On the whole, and under the conditions reported in these experiments, it appears as a reasonable conclusion that music is a slight distraction to a process of memorization, instead of being, as one might expect, a provider of background and context into which certain materials might be more readily assimilated than not.

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L'INFLUENCE DE LA MUSIQUE SUR LA MÉMOIRE

(Résumé)

On a fait quatre expériences différentes pour tester l'influence de la musique sur la mémoire. Dans la première expérience, la musique a servi comme condition préalable à l'apprentissage des paroles d'une connotation joyeuse ou triste. La musique a été une marche rapide, "Marche des Lilas", et une marche lente funèbre, "La Mort d'Ase". Dans les trois autres expériences, la musique a servi comme contexte pendant le processus de l'apprentissage. La musique employée dans la deuxième expérience a été la même que celle employée dans la première, et la matière à apprendre s'est composée de mots monosyllabes. Dans la troisième expérience, on a employé quatre sortes de musique, et on a appris des mots monosyllabes, présentés en séries au moyen d'un tambour de mémoire opéré manuellement, selon la méthode de l'anticipation. Les sujets ont rappelé et ont réappris après une période de quinze minutes, l'intervalle entre l'apprentissage et le rappel étant rempli d'une activité constante. On a appris de la poésie dans la quatrième expérience sous l'influence de deux différents types de musique. Le premier type de musique, "Le Largo", a été un

peu lent, doux, et mélodieux, tandis que le second, "L'Orage" a été tempétueux.

La musique comme condition préalable, a exercé un effet un peu détrimental sur l'apprentissage des mots joyeux et tristes. De même, dans l'apprentissage des mots monosyllabes, dans la deuxième expérience, la condition normale a montré une petite supériorité sur les conditions musicales. Quand la mémorisation a été accomplie par la méthode de l'anticipation, il y a eu le plus petit nombre d'épreuves dans la condition de la musique lente (La Mort d'Ase) et le plus grand nombre quand la musique a été une chanson en français. Les résultats du rappel et du réapprentissage ne montrent pas de différences signifiantes. Dans la mémorisation de la poésie, la condition normale est clairment supérieure aux conditions musicales. On a obtenu moins de résultats quand la musique a été de caractère tempétueux (L'Orage) qu'avec le plus doux type de rythme (Le Largo).

WHITELY

DER EINFLUSS DER MUSIK AUF DAS GEDÄCHTNIS

(Referat)

Man führte vier verschiedene Experimente durch, um den Einfluss der Musik auf das Gedächtnis zu untersuchen. In ersten Experiment diente Musik als Vorbedingung zur Erlernung von Wörtern, die eine freudige oder traurige Mitbedeutung haben. Die Musik war ein schneller Marsch: "Lilac March", und ein langsamer Trauermarsch: "Ase's Death." In den andern drei Experimenten diente die Musik als Kontextur während des Lernprozesses. Die im zweiten Experiment angewandte Musik war dieselbe wie die im ersten, und das Lernmaterial bestand aus einsilbigen Wörtern. Im dritten Experiment wurden vier Arten Musik gebraucht, und einsilbige, serienweise durch handgedrehte Gedächtnistrommel vorgeführte Wörter wurden durch die Anticipationsmethode erlernt. Die Versuchspersonen erinnerten und wiederholten das Erlernen nach einer Zeitspanne von fünfzehn Minuten, wobei die Zwischenzeit zwischen Erlernen und Erinnern durch konstante Tätigkeit ausgefüllt wurde. Im vierten Experiment wurden Gedichte unter dem Einfluss zweier verschiedener Arten Musik gelernt. Der erste Musiktypus, "The Largo," war eher langsam, sanft und melodiös, währenddem der zweite "The Storm" als sturmhaft bezeichnet werden darf.

Musik als Vorbedingung wirkte ein wenig nachteilig auf das Erlernen freudiger und trauriger Wörter. Ebenso wiesen im zweiten Experiment beim Erlernen einsilbiger Wörter normale Bedingungen einen Vorteil gegenüber den musikalischen Bedingungen auf. Wenn das Memorieren durch die Anticipationsmethode geschah, wurden am wenigsten Versuche gebraucht unter der Bedingung langsamer Musik (Ase's Death) und die grösste Zahl der Versuche, wenn die Musik aus französischen Liedern bestand.

Die Erinnerungs- und Wiedererlernangaben wiesen keine bedeutende Unterschiede auf. Beim Memorieren von Gedichten sind die normalen den musikalischen Bedingungen deutlich überlegen. Es wurden weniger Meldungen verzeichnet, wenn die Musik stürmischen Charakter (The Storm) hatte, als bei einem sanfteren Typus des Rhythmus (The Largo).

WHITELY

TACHISTOSCOPIC EXPERIMENTS ON THE PERCEPTION OF THE HUMAN FIGURE*

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NATHANIEL ROSS AND PAUL SCHILDER

Whereas reading has often been examined by the tachistoscopic method, more complex optic perceptions have rarely been objects of tachistoscopic investigation. Gestalt psychologists have for the most part examined rather uncomplicated shapes. Pötzl (10) was the first, so far as we know, to expose complicated optic figures tachistoscopically. Very often only parts and pieces of the whole were perceived. In dreams, parts which had escaped the perception came into consciousness. Pötzl came to the conclusion that in tachistoscopic perception the picture is cut into pieces and that the pieces may later on come into the foreground. Finally the pieces are again united into a whole. When the time of exposure is too short, the subsequent delivery of the so-far-unperceived pieces occurs in a dream and the pieces are not again united into a whole. A very similar type of perception, i.e., in parts, takes place in optic agnosia cases. The parts and pieces which subsequently come into consciousness do not form a unified whole but appear with parts of other perceptions which do not belong to the primary object of perception. Allers and Theler (1) exposed pictures tachistoscopically to normal subjects and found that parts of the pictures which were originally not perceived came into the consciousness of the subjects when association tests were made. Since then, Poppelreuter, Goldstein, and Gelb, neurologists, have frequently made use of the tachistoscopic method in the examination of cases of agnosia. [Cf., for instance, Last (7).] Systematic examinations concerning the perception of the human figure have not been made up to the present time, although in the Binet-Simon tests optic perception of the incompleteness of the human face or of the human body is made the object of investigation. Children

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with a mental age of seven years are aware of such an incompleteness of the human figure.

We have examined tachistoscopically the perception of the human figure because it seems to us that such perception has its particular place among the optic experiences. Schilder and Köhler have shown that one's own body is seen like any other object in the external world. Mach, in his *Analyse der Empfindungen,* has depicted "The Ego" when one is lying on the couch. It is a rather clear picture of one's own body. But, actually, the human figure is not so clear in our minds as one would expect. Certainly the optic body image (we shall use this term whenever we mean the impression we have concerning our own body and the bodies of others) is connected with tactile and kinaesthetic sensations if it is an image concerning one's own body. Dissociations are common. Parker and Schilder (9) have pointed out the dissociation between the different parts of the body image when one is riding in a fast elevator. Hartmann and Schilder have shown that the experience of one's own skin is not identical with the optically perceived or imagined surface of the body. In everyday life we rely upon the optic body image and adapt the experiences of other centers to the optic experience. Schilder has shown that the double vision of one's own finger, produced by voluntary squinting, also has an influence on tactile perception. Two touches are experienced (one on each finger) when the finger is touched.

We experience our body generally in the so-called normal position. We have a tendency to ignore actual deviations from this normal position. Ross (11) has shown that the self-perception of one's own face with closed eyes is dependent upon our optic expectation. We arrive thus at the paradoxical formulation that one's own body is not immediately experienced as a gestalt, but its shape has to be created according to the total situation. The body image is not a stabilized structure. If one crosses the hands and intertwines the fingers, the body image becomes completely confused (Japanese illusion). Only the renewed touch of a specific finger or small movements of the fingers make a correct reconstruction of the body image possible. The completeness of the body image is guaranteed only when there is a normal functioning of the vestibular apparatus. At the starting and at the stopping of a fast elevator a part of the substance of the body emanates as phantoma from the body image which

is based on optic and tactile experiences. Schilder has shown that sadistic impulses destroy the body image. There is a close relation between the perception of one's own body and the bodies of others. Parts of the body images of others are taken into our own body image. Parts of our body image are projected into others. Whenever children are especially interested in a part of their own bodies (especially when the shape or function of parts of the body is inferior) they are also interested in the same parts of others. The difficulties involved in right and left orientation which one finds in some types of agnosia are always present with respect to one's own body as well as with respect to the bodies of others.

By means of the mirror and by interest in others we arrive at the perception of those parts of our own body which we do not perceive immediately in an optic way. How strongly the optic picture influences the perception of one's own body can be seen in the well-known illusion of the floating finger. If we hold the extended index fingers before the eyes so that they are 2 or 3 cm. apart, and produce double vision by squinting, at the place where the images of both eyes meet there appears a round or oval object, the ends of which are formed by the finger-tips. A shadowy finger appears which is the continuation from the finger-tips to the base of the finger. Finally, the clear picture of both index fingers follows, which is felt as part of the living body. The round object in the middle, the floating finger, which appears in vivid colors and as a real object, is not felt as a part of the body in spite of the fact that both eyes contribute to the perception. The index fingers which are experienced as parts of the body have the advantage of belonging to the optic continuity of the body.

From the preceding discussion it appears that an investigation concerning the optic experiences in the perception of the human body promises interesting results. For our study we chose the tachistoscopic examination of incomplete or disfigured human figures. Our interest in this particular problem was provoked by some experiences of Schilder and Kinder with children with reading disability. Along with the usual material which we presented tachistoscopically to these children we presented also some incomplete human figures. After such presentations the children often reported the perception of a complete figure. We soon came to the conclusion that such tendencies to *totalisierenden Gestaltauffassung* (Poppelreuter) are also present in normal children and in adults.

For our study of this phenomenon we used a tachistoscope which consisted of a box from which the light was excluded by a camera shutter. Observations were usually binocular, but with those subjects in whom double vision occurred in binocular vision monocular observation was used.

After a great number of preliminary experiments we finally chose the following series of simple drawings for the final experiments:

1. Hand with middle finger absent.
2. Naked woman—frontal view.
3. Head from which left eye, ear, and left part of the hair are absent.
4. Head from which the left half of the face from the eye downward is absent.
5. Woman's legs with shoes.
6. Legs of a standing woman; on the left leg a part below the knee is absent. (Figure 1.)
7. Female face; the left half from the mouth upward is missing.
8. Complete female face.
9. Female face with complete chin; the other part of the left side of the face missing completely.
10. Naked female figure with three arms; one left arm is bent, the hand is lying on the breast. The other arm is stretched. (Figure 2.)
11. A schematic female face; on the forehead is a third eye. (Figure 3.)
12. Man whose nose is increased in size and has tumor overgrowth. The nose is a little darker than the surrounding parts.
13. Man who is threatening with raised arms. The left arm is absent. The legs are spread apart. His mouth is wide open. He wears a black vest-coat with a broad black belt. The lower parts of the long trousers are dark.
14. Boy in the position of a discus thrower. The left arm is stretched; the right arm is absent. He has short black trousers. (Figure 4.)
15. Boy with legs spread apart, hands in pockets; left leg absent. (Figure 5.)
16. Dog with long ears, short tail, face turned toward the observer; left hind leg missing. (Figure 6.)
17. Head of a young female, profile view, eyebrows present, eye missing. (Binet-Simon test figure.)
18. Old-fashioned woman, arms missing. (Binet-Simon test figure.)
19. Elderly woman without nose. (Binet-Simon test figure.)
20. Head of a boy, half profile, mouth missing. (Binet-Simon test figure.)
21. Boy with head missing.

The pictures were exposed in different sequences. The time of exposure was at first always 1/100 second. When it was found that the picture was not always recognized, a second and third exposure

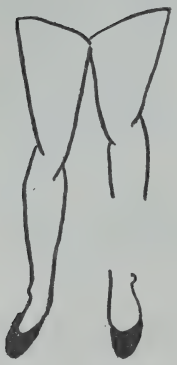

FIGURE 1
PICTURE 6

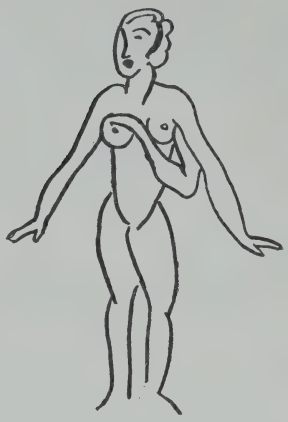

FIGURE 2
PICTURE 10

FIGURE 3
PICTURE 11

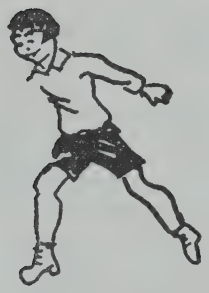

FIGURE 4
PICTURE 14

FIGURE 5
PICTURE 15

FIGURE 6
PICTURE 16

was given, sometimes immediately after the first exposure and sometimes after other pictures had been exposed. In some cases the second exposure was even postponed to the next day. If perception with the 1/100-second exposure time failed persistently, exposures of 1/50, 1/25, 1/5, and 1/2 second followed. At the end of the experiments, pictures which had not been recognized were shown to the subject in an indefinite exposure and the subject was asked to compare the tachistoscopic experiences with the full perception.

The subjects were two children 9 years of age, Charles O and Stephany Bi; four women, Si, Be, Ba, Wo, all between 20 and 35

years of age; and three men, Pa, Go, and Br, all between 30 and 40 years of age. The majority of the adults were physicians. The Binet-Simon figures were shown only to those to whom they were unknown.

In the following protocols the time of exposure was 1/100 second, unless otherwise stated. The numeral on the left indicates the figure tachistoscopically exposed.

Subject: Charles O

17. I didn't see anything.
17. A woman with waved hair. She looks to the right side. (What about her eye?) It is there. (Nose?) Yes. (Mouth?) Yes. (Chin?) Yes. (Ears?) Yes, one ear.
17. (Be attentive concerning the eye.) I see only one eye.
20. I see the head of a woman. She had only hair. No eyes. No nose. No mouth. (Were they present or not?) I don't know. (Ears?) No.
20. I saw this time only a small black spot.
20. I see the face of a girl which looks to the right. She has eyes, nose, a mouth, and a body up to the middle of the chest. Her hair is combed backwards.
20. I now see a man who looks the same. Two eyes, hair combed backwards. I don't think it is the same picture as before. I do not think he had ears, but he had a mouth, chin, and nose.
20. He had an eye, the half of a nose. I saw his hair. (What about his mouth?) The half.
15. I saw a little point. It was black and round like a black ball.
15. The leg of a man up to the stomach.
15. The leg of a boy with a black spot near the middle. I didn't see the foot, only the blot.
15. A girl—her legs are standing together. She has the arms akimbo. (Why is it a girl?) Her hair is combed back. (Is it the same picture you saw just before?) No, here her hair was combed to the side so that I only saw one eye, the half of a mouth, and the half of a nose.
15. I have seen the same black round spot. I saw a boy in a similar way like the girl. (Arms?) I saw them from the side so that I saw only one arm. I did not see the legs.
10. It was a woman who stretched her arms to the sides. She wore a dress.
10. (Look at her breast.) I saw that she was naked.
14. I didn't see anything. A sitting woman, dressed, looking to the left side. (Head?) Yes, with hair on it. (Arms?) I didn't see them. (Legs?) They were bent in the knees.
14. A small round spot.
14. A fat man who steps forward with one leg. One arm is stretched forward. I couldn't see the other one because it was on the side.

11. The face of a woman looking straight forward. She has a nose, two eyes. On the nose there was powder. I saw the neck and the mouth.
11. The mouth was open. (Forehead?) It looked like a bump. She looks like a servant girl. She wears a maid's bonnet.
11. (Look at her forehead.) She has a black mark there.

The experimentation continued after a pause of a quarter of an hour.

17. It is the same woman. She has a mark on her forehead. She has the mouth closed and looks straight forward.
17. Another woman who looks at the right side. She has waved hair, one eye, half a nose, and half a mouth.
17. (Look at her eye.) I saw it—an eyebrow was above. (Did you ever see this picture before?) No.
20. Another woman. Her hair is combed back. She looks to the right side. (Ear, eye, nose, mouth?) Yes.
20. (Look at the mouth.) She has none.
15. I see a child walking, one leg is before the other. I see only one arm on its side. He stands on his soles. The legs are straddled.
10. I see a woman without outstretched arms. She was naked but with drawers. (Legs?) I did not see them.
10. (Look at her chest.) She was naked, that's all.
14. A boxer, running, one arm is outstretched and one arm is backward.
14. I have seen only one arm. (What about the other?) I didn't see it. (Did you see this picture before?) No.

The pictures not recognized up to this point were then shown to the subject in an indefinite exposure.

17. I thought that the eye was below the eyebrow.
20. I remember this picture. I did not see that the mouth was absent.
15. One leg is absent. I thought he runs. I did not see that his face looks forward.
11. I never saw that before. She has three eyes.
10. She has three arms. I did not realize it.
14. I thought he runs. I did not see the one arm is missing.

These protocols are interesting in many respects. Pictures 20 and 17 are not always seen as the same picture in subsequent exposures. The subject emphasizes that we are dealing with different pictures. It is true that perseveration plays a part when the patient disclaims the identity between Picture 17 in the present and Picture 17 in the previous exposure. The first tachistoscopic exposure was marred by a picture previously exposed.

When Pictures 14 and 15 are exposed the subject has a vivid im-

pression of movement. In Picture 15 the black trousers are the only part of the figure which is perceived and they take the form of a circle (ball).

In Picture 17 the subject sees the missing eye. This illustrates what we shall call (*A*) *the principle of substitution of missing parts.* This is again illustrated when the subject sees the missing mouth in Picture 20.

In Picture 15 the completion takes place with a reinterpretation of the picture. The subject sees the picture in a perspective way so that the absence of the leg appears natural. This is an example of (*B*) *the principle of the substitution of missing parts with reorganization of the total picture.*

In Picture 20 a compromise takes place between the completion tendency and the actual perception. The subject sees half of the mouth. We call this (*C*) *the principle of compromising completion.* When in Picture 20 the subject sees a second ear, apparently on the basis of his knowledge, we have an example of (*D*) *the principle of completion according to knowledge.*

When Picture 10 is exposed the subject sees the naked figure clothed in accordance with the demands of his sense of modesty. This we speak of as (*E*) *the principle of completion according to complexes.*

In the perception of Picture 20 our subject substitutes the mouth but does not see the ear. This illustrates what we shall call (*F*) *the principle of the transposition of the defect.* Another example of this principle is found in another exposure of the same picture when the subject sees half of the nose.

We did not find in the case of Charles O any indication of a mechanism which appears to be of importance in other cases, i.e., the appearance of the defect in an exaggerated way—(*G*) *the principle of exaggeration of defects.*

In the tachistoscopic exposure of Picture 10 the supernumerary limb is not perceived—(*H*) *principle of omission of supernumerary limbs.*

In our Picture 11 the subject does not see the third eye at all at the first exposure. Later on he misinterprets it as a bump. This is an example of (*I*) *the principle of reinterpretation of supernumerary parts.*

The protocols show that perception becomes more accurate when

attention is directed to a specific part of the picture. Attention therefore seems to have a facilitating influence on perception.

Another factor which seems to enter into tachistoscopic perception is a strong tendency to perseveration. Under the influence of this factor the subject almost overlooks the fact that Picture 17 has been substituted for Picture 11. After the exposure of Picture 17 (girl's head) the boy in Picture 20 is persistently called a girl.

Stephany Bi

19. A face which looks to the right side. I saw mouth and nose but not hair and eyes.
19. A face which looks to the right side. Two eyes, black hair, nose. I didn't see the nose well, I saw the mouth.
19. (Look at the nose.) I see it. I saw the ear, two eyes.
19. (Look at the nose.) It is small, I see it.
6. Legs of a woman. Half a body. Pointed shoes.
6. I still see it. A small part under the left knee is missing.
18. A whole woman, black lines on her body. I couldn't see the head. No legs. I didn't see her arms.
18. (Look at her arm.) She has none. She has no nose. No mouth, a round head. I didn't see any legs. (Is she dressed?) Yes, a long frock.
18. (Look at her face.) She has a mouth, nose and ears.
17. Woman with black hair. Looks to the right. Has nose and mouth, one eye and one ear.
17. She has an ear, no body. (Nose?) Yes. (Mouth?) Yes. (Eye?) I don't know.
17. (Look at the eye.) She has none. Everything else is present.
15. I saw two legs with pointed shoes and short black trousers. Otherwise nothing. The legs were straddled.
15. I saw a head with hair parted. It was a boy, I saw his arms. I didn't see anything else. (Legs?) In the same position as before.
15. He is sitting and his legs are turned to the front. He has mouth and eyes.
15. (Look at the legs.) I saw only the right one.
14. A boy running. One arm is forward, the other one pointing backward. I can see both and both legs. He has short black trousers.
11. A woman with long hair, black, two eyes, one nose, one mouth, two ears. (Forehead?) Yes.
11. (Look at the forehead.) I didn't see it this time, I saw her eyes and her nose.
11. I saw her ears again. (Forehead in order?) Yes.
20. A man, two lines which mean his neck. I saw nose and mouth. Not the other things. He looks straight forward.

20. He has black hair and something else nearby. (Eyes?) Yes. (How many?) Two, and he has two ears. (Mouth?) A little one.
20. (Look at the mouth?) The face looks to the right side, the mouth is round.
20. (Look carefully at the mouth.) It is round, I saw only three teeth.
16. It is a dog, a wolf, two ears which stand upright. A round mouth, a large tail. (In what direction did the dog look?) I don't know.
16. The head looks to the right side. He has two eyes, one nose, one mouth. (Legs?) I saw his toes.
16. (Look at its legs.) I didn't see anything.
16. He has five toes on each leg.
16. (Look at the hind legs.) I saw two, the tail goes up.
16. (Look at the hind legs.) I saw two.
4. It was a man. I saw only one half. One eye and one ear, one nose and one mouth. (A whole mouth?) Yes, and black hair.
4. Two eyes, half a nose, half a mouth. He looked straight ahead.

Continuation after a pause:

19. I saw a man with a small chin. (Nose, mouth, hair?) I did not see them.
19. He looks to the right side. Has black hair, two eyes, two ears. He looks like a boy. Has a big head. (What else?) A mouth, two eyes, a nose.
19. (Look at the nose.) There is a space in between, but everything is all right.
14. I hardly saw anything. Something of his nose and his chin.
14. He runs again. He has a vest-coat. (Arms?) Yes, and two legs.
14. (Look at his right arm.) He has none, I saw it before.
11. The same woman I saw before. She is funny, big eyes and a big nose and a big mouth. (Look at her forehead.) She has a small curl in the middle.
11. (Look at the forehead.) I saw only her chin and a little point on it.
11. (Look at her forehead.) I saw a long curl.
20. I didn't see anything.
20. I saw this girl twice. Black hair, two eyes, one nose, one mouth. She looks to the right side.
20. She has only one ear.
20. (Look at the mouth.) She has a nose but no mouth. I can't see her eyes.
16. I see a wolf. I saw it before. Its tail goes up, its ears too, its body is round. I did not see the hind legs, I saw only the forelegs. (Look at the hind legs.) One is before the other.
16. (Look at the hind legs.) There is only one.

The pictures are now shown in indefinite exposure.

19. I never saw it before. She has no nose.
4. I just saw it as it is.

14. I saw two arms at first, but he has only one.
11. I saw it; she has three eyes, I thought it was a curl.
20. I saw it; he has no mouth. (Did you realize that before?) No.
15. I never saw it before. One leg is missing.
17. I never saw that before. She has only one eye.
18. I saw it already. Her head is bent forward, a part of the nose is absent. She has only one ear. She has buttons and a shirt. (What is missing?) The pockets. (Arms?) She has none.

There is no doubt that nearly all the remarks concerning the observations of Charles O apply also to those of Stephany Bi. A few points of resemblance stand out especially clearly. Charles O perceived only a small part of the figure in Picture 20. This incompleteness of perception is obvious in the reports of Stephany Bi concerning the exposures of Pictures 11, 15, and 19. These reports also substantiate Pötzl's observations that in subsequent exposures the parts which were not perceived in the earlier exposures come into the foreground.

In this case also we find the same effect of attention and attitude which was observed in the first case.

Of practically all the principles which we mentioned above in connection with the first case we find instances in the protocols of our second case—(*A*), in the reports of Pictures 15, 17, 19, and 20; (*B*), in the reports of Pictures 14 and 15; (*C*), Pictures 19 and 20; (*D*), 16, 19, and 20; (*E*), no instance; (*F*), Picture 19; (*G*), Pictures 4, 11, and 18; (*H*), Picture 11; (*I*), Picture 11. This patient sometimes completed the picture with a feeling of uncertainty. We shall call this (*Aa*) *the principle of substitution with uncertainty*. We also found with this patient that erroneous substitution may have a perseverative after-effect. In such a case we speak of (*Ab*) *the principle of after-effect of erroneous substitution* (e.g., in the reports of Pictures 20 and 16). Supernumerary parts which are not originally perceived may influence the subsequent perception. This carry-over we speak of as (*K*) *the principle of the after-effect of misinterpreted supernumerary parts.*

We shall now reproduce the protocol of an adult, Pa. Special emphasis was placed on the question of the certainty of the patient regarding his statements.

Pa

12. Bust of a man. The nose is swollen.
10. Sketch of a woman, breasts are hanging down. Arms abducted. Left breast deeper than the right.

15. Small boy. Either he runs or the left leg is absent.
20. The right side of the face is swollen.
20. The same.
19. Female profile.
19. The same. The lips are thick. She is of middle age, she has a double chin and a short nose.
19. I see the lower half better. (Which impressions are sure?) Hair, chin, age. (What is uncertain?) Her individual age.
14. Similar to the previous picture. Both legs are present. The position is different. He jumps from the right to the left side. I can't remember the position of the arms. (What do you remember about the arms?) The right arm was bent, maybe the left arm was stretched. Maybe that is only the effect of the stretched left leg.
14. This time I saw it better. Left arm and left leg are outstretched. He does not jump but stands on the right leg; the right arm is bent. (Are you sure?) Yes, I think I saw it.
16. An animal that looks at me like a fox. It is not complete, but I don't know in what way.
16. Maybe the right hind leg is absent.
18. Female figure dressed in an old-fashioned style. I did not see the face, only the dress. Maybe the face is not there, but I am not sure. I did not see arms. (Are they present?) I don't know.
18. No arms. The face is incomplete, but I don't know how. There is no particular expression in her face.
18. I did not see the face.
18. (Look at the face.) I did not see it clearly.
17. Profile of a young woman, the base of the nose is there, maybe something else, but I don't know.
17. (Describe details.) The chin is certainly there. Well formed. (Mouth?) Present. I am not quite sure. (Tip of the nose?) Present. I am sure that the other part of the nose is absent. I am not sure concerning the eye, I saw only one point. The hair is combed backwards; I can't remember that I have seen an ear.
17. I saw only the ear and didn't give any attention to the rest.
17. The mouth was drawn downwards.

After a pause:

19. The same profile. The ridge of the nose is absent, its tip present.
20. Something is missing in the left part of the face. (Ear?) I don't know whether it was present. There are transverse lines over the face. I am not sure concerning the left ear. (What about the eye?) There is something.
20. Maybe the left half of the mouth is absent.
14. The right leg is bent a little. I don't know where the right arm is, maybe it is absent. (Face?) Raw.
14. The right arm is absent, the face is complete.

This adult very often has a feeling of uncertainty concerning his observations. In these protocols, as in the others, it is clear that

the direction of attention is of great importance. It is also clear that parts which are not perceived in the first presentation come into the foreground at the next presentation (e.g., in the records for Picture 17).

This subject has the tendency to see movement in Pictures 14 and 15.

Since the number of experiments with this subject is limited, we do not find all of our principles illustrated in his observations. We do find principle *A* shown in his reactions to Picture 20; principle *B,* in his perceptions of Picture 15; principle *C,* in the records for Picture 18; and principle *I,* in the records for Picture 10.

It is significant that defects are very often perceived without any specification. This fact we have designated (*N*) *the principle of perception of a defect without localization.* It is also interesting that at the exposure of Picture 19 the defectiveness of the nose escapes attention, but that, at the subsequent exposure of Picture 17, the subject persistently sees a defectiveness of the nose. This is an example of another principle—(*L*) *the principle of perseveration of a non-perceived defect.*

Observations of the other adults we shall present in abbreviated form. Subject Wo requires a long time for the realization that incomplete pictures are being shown. Following this realization she perceives the defects generally well. Her observations show our principle *A* in her perception of Pictures 20, 17, and 14, principle *F* in Picture 17, principle *G* in Pictures 14 and 15, and principle *I* in Picture 11.

Subject Br shows principle *A* in her perceptions of Picture 19, *B* in Picture 15, *C* in Picture 20, and *F* in Picture 20. She sees a complete figure, but sees the line of the neck only incompletely. This is an instance of completion with transposition. In Picture 19 she sees spectacles, an instance of our principle *E.* Her perception of Picture 21 shows the influence of principle *G.*

In the observations of subject Si we find examples of principle *A* in her perceptions of Pictures 14 and 17, of principle *Aa* in Picture 1, of principle *C* in 20, of principle *F* in 15 and 20, and of principle *I* in 21. It is very remarkable that at the exposure of Picture 10 the patient did not at first perceive the one supernumerary arm, while at the next exposure the other supernumerary arm was not seen. This is an example of yet another principle,

i.e., (*Ha*) *the principle of successive elimination of supernumerary limbs.*

Subject Go's observations show instances of principle *A* in Pictures 13, 14, and 15, principle *B* in 13, 14, and 15, *E* in 11, and *I* in 10. One of the pictures provokes vivid childhood impressions. Principle *B* is shown in his perceptions of Picture 16. He sees at first only a part of the dog: "Ear, face, nose, and a part of the back are absent. Maybe the tail and the legs are present. But anyhow I couldn't see more than three and couldn't differentiate the legs from the other parts of the body."

Subject Be complains that she forgets the pictures too quickly. Instances of principle *A* are seen in her perceptions of Pictures 20 and 6, *B* in Picture 20, and *E* in 13 and 11. At the third exposure of Picture 11 she disowns what she has perceived correctly: "There is something oval on his forehead. It has the shape of an eye. But I do not think that it was an eye." We speak of this as (*M*) *the principle of disowning of the perception of a supernumerary part.*

6. Two female legs in short trousers. (Complete?) Yes, as far as I have seen.
6. This time I saw the line interrupted on both sides. I saw the right side better.
6. The left leg is not as complete as the right one. The left was still more in pieces, as before.
6. Complete on the right side, on the left side less complete, as before; the ankle is missing and something else too. (Principle *G*.)

This subject very often sees lines in fragments.

Subject Ba showed examples of our principles in the following instances: *A* in her perceptions of 4, 6, and 13, *B* in 4, 10, and 16, *G* in 4, *Fa* in 6, and *K* in 16.

Discussion

We may now summarize our findings. At the tachistoscopic exposure very often a total impression is formed at first. Stephany Bi says, for instance, "I saw only trousers but I knew that there was a complete figure." It is remarkable how often the subjects give special attention to the question of whether the exposed picture represents a man or a woman. They also often remark about the emotional state of the person represented in the picture in spite of the fact that the pictures themselves are not suggestive of such

states. Another general finding is that one picture when given successive exposures is very often judged to be two different pictures. This is especially true with children, but it also occurs with adults. With some of the pictures the subjects get a vivid impression of movement, although the drawings are not intended to give suggestions in this direction. Seemingly, the movement forms an important part of the total perception.

Occasionally the subjects see an outstanding piece of the picture as an independent whole. In contrast to this type of perception, they very often see only pieces of the picture, or only single parts. (We speak of pieces when a part of the picture is taken out which has no definite meaning as a part, but we speak of parts when the subjects see an isolated ear, nose, eye, etc. These are real parts of the face, but not, for example, a piece of the cheek.) Sometimes the subjects have the impression that they see only the part at which they look directly. This, however, is probably not the case. Charles O, with Picture 20, sees only the hair of a female head. Very often not pieces but parts are seen clearly. For the most part, perception is better when the attention of the subject is drawn to a particular part of the picture.

That which stands out clearly at one exposure is very often less clear at the next exposure.

In general, we may say that there is a tendency to perceive the pictures in parts as well as in pieces.

Most of our subjects complain that they could not see everything because of the short duration of the exposure. Some of them feel sure that they have seen more than they can remember. They have the impression that they forget too easily what they have seen tachistoscopically. In the preliminary experiments Schilder felt very strongly in this way.

Tachistoscopic perceptions often have characteristics similar to those of eidetic images. Our protocols show that there is a strong tendency to perseveration of these perceptions. They are also very likely to call to memory early experiences of the subject.

In connection with the tachistoscopic studies of reading, the question has often been raised as to whether the perception starts with an impression of the total form of the word or with the perception of outstanding letters (parts). Our results do not offer a solution to this problem, but they do suggest that both possibilities may occur.

The results reported thus far are fairly representative of what is known about tachistoscopic perception. However, we wish especially to emphasize the importance of the tendency to see only parts and pieces of the total picture. Pötzl pointed out this tendency and Allers and Theler also observed it in their study. This fragmentation of tachistoscopic perceptions shows the same formal characteristics which Schilder and Klüver found in eidetic imagery. Identical phenomena may be observed in optic agnosias, in dreams, and in optic images (3). Another characteristic common to dreams, eidetic images, representation, and tachistoscopic perceptions is that they are very easily forgotten. From such similarities, one may develop the hypothesis that the physiological processes underlying these activities are also similar.

We turn now to the discussion of the specific problems concerning the tachistoscopic perception of the human figure.

In our protocols we have found the following principles represented:

A. Substitution of missing parts.
Aa. Substitution with uncertainty.
Ab. After-effect of erroneous substitution.
B. Substitution of missing parts with reorganization of the total picture.
C. Compromise completion.
D. Completion according to knowledge.
E. Completion according to complexes.
F. Transposition of the defect.
G. Exaggeration of the defect.
H. Omission of supernumerary limbs.
Ha. Successive omission of supernumerary limbs.
I. Reinterpretation of supernumerary parts.
K. After-effect of misinterpreted supernumerary parts.
L. Perseveration of a non-perceived defect.
M. Disowning of the perception of a supernumerary limb.
N. Perception of a defect without localization.

Most of these principles we have found illustrated in the protocols of most of the subjects. That we did not find all the mechanisms in all subjects is probably due to the rather limited number of experiments with a single subject. However, it is true that we found only in children that substitution takes place merely on the

basis of knowledge. It is generally known that children do not differentiate perception and knowledge as well as adults do. Completion according to complexes (in the sense of a general affective attitude) also comes to a clearer expression in a child.

In general, there is a strong tendency to see the human figure (and also the animal) as a complete totality. With some of the subjects repeated exposures are necessary in order to bring to them the possibility of thinking of a defect in the exposed figures. Our preliminary questions regarding this matter have led us to believe that the degree of assurance concerning the erroneous completion does not differ from the degree of assurance concerning the correct tachistoscopic perception. When the incompleteness of the picture becomes too obtrusive the subjects reinterpret it—they either say that the unseen parts cannot be seen in this particular position, or they suppose that the vivid movement covers single limbs (Pictures 14, 15, 16).

The struggle between the obtrusive perception of the irregularity and the tendency to see a total unit often leads to compromise—a little mouth or a half a nose is seen. Compromise of this type is seemingly possible with the help of the fact that the primary impression of an incomplete picture is not based upon a specific perception of the defect but upon the general impression of defectiveness, the localization of which is not possible. We find, also, that the defect is very often transposed to another position. It is as if the defect would float freely. Without the use of special methods it is impossible to decide what determines the final localization of the defect.

The impression of defectiveness may become overwhelming and the subject may get an exaggerated impression of the defect. The incompleteness of the left leg in Picture 6 was seen by several subjects as also related to the right leg. One should not forget that the subjects have certainly perceived a greater part of the pictures than they themselves know. One subject does not perceive a defect of the nose in one picture but sees the same defect persistently in another picture which was exposed later.

We may draw the conclusion that perceptions go through a process of development which decides which parts of the raw material shall be used and in which place the parts shall be used.

Supernumerary limbs or parts are very often overlooked. In the

one case the one limb was overlooked at the first exposure; in the other case, at the second exposure. There can be no doubt that central factors determine the selection. The supernumerary eye is perceived as a curl, the supernumerary arm as an enlarged breast. When the supernumerary limb is finally perceived, the subjects still try to disown the experience. These facts certainly point to the existence of a tendency to coordinate the tachistoscopic perception with experience. One may say that the normal and complete is more frequent than the defective or supernumerary. We therefore expect the normal. It is difficult to deny the influence of experience. Experience creates gestalten (configurations) and gives to the parts and pieces of the gestalt an inner coherence. It creates the so-called factors of coherence. There is no question, also, about the fact that we want to live in a well-ordered world with a well-regulated average experience. In this respect, gestalt is an average which is based upon the structure of the objects and their inner coherence. We wish especially that our bodies be normal, average, and well regulated. This interest in the integrity of one's own body is the expression of self-love—we call it narcissism in psychoanalysis. Since there are close relations between one's own body and the bodies of others, we may expect that there exists a vivid general interest in the integrity of the bodies of others. There are also gestalten, the integrity of which is protected by our emotional attitudes. It is difficult to prove that this factor is of importance in the experiments we have discussed since it is impossible to compare the results concerning the completion of human figures in tachistoscopic experience with the results concerning inanimate objects.

In apparent contradiction to the tendency to see a complete and well-regulated totality in the human figure, we have shown that a defect may be perceived in an exaggerated way. We have also emphasized the fact that very often parts and pieces are perceived instead of wholes. We seem to be dealing with a principle in psychic life which is opposed to the tendency to come to well-regulated totalities. It is a principle of perception in parts and pieces and of fragmentation of experiences. Klüver has spoken of a configuration in quantums. Kanner and Schilder have pointed out the fragmentation and destruction of gestalten which takes place in optic imagination and have brought them into relationship with destructive psychic tendencies. It seems that even in experiments as simple as the ones here reported two important psychic tendencies are reflected.

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DES EXPÉRIENCES TACHISTOSCOPIQUES SUR LA PERCEPTION DU CORPS HUMAIN

(Résumé)

On montre au moyen du tachistoscope des dessins simples qui représentent des corps humains à des sujets normaux. Plusieurs de ces corps montrent un manque de parties ou de pièces, d'autres montrent des membres supplémentaires.

Les expériences montrent une étroite relation entre la représentation optique, les images eidétiques, les tableaux de rêves, les perceptions agnostiques, et les perceptions tachistoscopiques. Il est probable qu'il s'agit de processus

semblables dans le système nerveux dans tous ces cas. Dans ces expériences on a pu trouver les principes suivants:

A—substitution des parties qui manquent; *Aa*—substitution avec incertitude; *Ab*—effet ultérieur de la substitution erronée; *B*—principe de la substitution des parties qui manquent avec une réorganisation du tableau total; *C*—complétion de compromis; *D*—complétion selon la connaissance; *E*—complétion selon les complexes; *F*—transposition du défaut; *G*—exagération du défaut; *H*—omission des membres supplémentaires; *Ha*—omission successive des membres supplémentaires; *K*—effet ultérieur des parties mal interprétées; *L*—perséveration d'un défaut non perçu; *M*—dénégation de la perception d'un membre supplémentaire; *N*—perception d'un défaut sans localisation.

Il y a une tendance générale à voir le moyen et le régulier. La gestalt est à cet égard le moyen, le régulier et le normal. On s'attend par expérience à rencontrer le moyen et le normal mais on veut la normalité pour des raisons émotives, surtout la normalité de son propre corps et du corps d'autrui. Il y a une tendance à la totalité et à l'ordre, mais on trouve dans la vie psychique, et surtout dans les expériences ici décrites aussi, des perceptions en parties et en pièces et des impressions fragmentaires en général. Il est possible que ces tendances à la fragmentation aient des rapports avec les tendances destructrices de la vie émotive, mais en tout cas il existe une tendance formelle à détruire et à rendre fragmentaires les perceptions totales et les gestalten.

Ross et Schilder

TACHISTOSKOPISCHE EXPERIMENTE ÜBER DIE WAHRNEHMUNG DER MENSCHLICHEN GESTALT

(Referat)

Einfache Zeichnungen, die menschliche Gestalten darstellen, werden normalen Versuchspersonen tachistoskopisch gezeigt. Viele dieser Figuren weisen teilweise oder stückweise Defekte auf, andere Figuren haben überzählige Glieder.

Die Experimente zeigen eine enge Beziehung zwischen eidetischen Bildern optischer Darbietung, Traumbildern, agnostischen Wahrnehmungen und tachistoskopischen Wahrnehmungen. Es besteht eine Wahrscheinlichkeit, dass es sich in allen Fällen um ähnliche Prozesse im Nervensystem handelt. Man konnte in diesen Experimenten die folgenden Prinzipien finden:

A—Ersetzung der fehlenden Teile; *Aa*—Ersetzung mit Ungewissheit; *Ab*—Nachwirkung fälschlicher Ersetzung; *B*—Prinzip der Ersetzung fehlender Teile mit Reorganisation des ganzen Bildes; *C*—Kompromiss-Ergänzung; *D*—Ergänzung auf Grund des Wissens; *E*—Ergänzung nach Komplexen; *F*—Versetzung des Defektes; *G*—Uebertreibung des Defektes; *H*—Auslassung überzahliger Glieder; *Ha*—Nacheinander folgende Auslassung überzähliger Glieder; *I*—Wiederdeutung überzähliger Teile; *K*—Nachwirkung von falsch gedeuteten Teilen; *L*—Verharrung (perseveration) von nicht wahrgenommener Mangelhaftigkeit; *M*—Nichtanerkennung der Wahrnehmung überzähliger Glieder; *N*—Wahrnehmung eines Defektes ohne Lokalisation.

Es besteht eine allgemeine Tendenz den Durchschnitt und das Ordentliche zu sehen. Die Gestalt ist in dieser Hinsicht der Durchschnitt, das Ordentliche und Normale. Durch Erfahrung erwarten wir dem Durch-

schnittlichen und Normalen zu begegnen, aber wir wollen die Normalisierung auch aus emotionalen Motiven, besonders die Normalisierung des eigenen Körpers und der Körper anderer. Es besteht eine Tendenz zur Totalität und Ordnung, aber wir finden im psychischen Leben und besonders auch hier bei diesen Experimenten Wahrnehmungen in Teilen und Stücken und fragmentarischen Eindrücken im Allgemeinen. Es besteht eine Möglichkeit, dass diese Tendenzen zur Fragmentierung eine Beziehung haben zu den Zerstörungstendenzen im emotionalen Leben, aber es besteht jedenfalls eine formale Tendenz totale Wahrnehmungen und Gestalten zu zerstören und zu fragmentieren.

Ross und Schilder

TEMPORAL FACTORS IN THE FORMATION OF CONDITIONED EYELID REACTIONS IN HUMAN SUBJECTS*[1]

From the Psychological Laboratory of Oberlin College

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I. INTRODUCTION

The object of this investigation was to study the formation of conditioned eyelid reactions in human subjects with reference to the influence of the time relations between the conditioned and the unconditioned stimulus upon the amount and the character of the conditioning. That temporal factors are significant in conditioning a reaction has long been emphasized (6, pp. 26-28). A review of the literature shows, however, that there is a paucity of adequate comparative data concerning the problems involved.[2]

The present status of the problems of the time relationship may be summarized as follows: The possibility of backward conditioning is definitely established, but the relative ease of such conditioning is a matter of dispute (2, 3, 7, 11, 13, 14). Investigators are agreed that a most favorable time relation exists when the conditioned stimulus precedes the unconditioned stimulus by an interval greater than some critical length of time (4, 9, 13, 14); that, beyond this critical interval, the amount of conditioning is a function of the time interval between the stimuli has been suggested (13, 14). The relative importance of the factor of precedence in time of either of the stimuli as compared with other factors remains an open question.

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[1]The author wishes to thank Professor L. E. Cole, under whose direction the work was done, for his active interest and his many significant suggestions during the course of the investigation. Mr. J. M. Snodgrass of the Oberlin Laboratory devised the apparatus which made a careful control of certain of the experimental conditions possible, and Mr. Joseph Miller gave valuable assistance in securing the records.

[2]For detailed summaries of previous work, see Switzer (11) and Wolfle (13, 14).

In the present experiment, the performances of groups of subjects in nine time intervals, ranging from that in which the conditioned stimulus *preceded* the unconditioned stimulus by 1.48 seconds to that in which the conditioned stimulus *followed* the unconditioned stimulus by .90 seconds, were studied. One hundred one subjects served in the investigation, each subject being used in a *single* time interval. All measurements were taken at a single session. The data secured have served as the basis for generalizations concerning the relative ease of backward conditioning, the relative importance of the precedence in time of either of the stimuli as compared with the influence of the relative strength of stimuli, and the question of the influence of the *time interval* between the stimuli upon the formation of a conditioned reaction in human subjects. Certain other conclusions derived from the data are presented since they concern the specific problems at hand.

II. Technique

In the present experiment, an electric shock of constant intensity and duration, applied to the skin surface above the infraorbital nerve of the maxillary division of the trigeminus as it emerges through the infraorbital foramen, was used to elicit the lid-reflex. The conditioning stimulus was a click in the headphones worn by the subject.

Apparatus. The set-up devised had two objectives:

1. The absolute constancy of the stimuli employed.
2. The elimination of extraneous stimuli.

The unconditioned stimulus. A small circular electrode (negative), 1.5 cm. in diameter, was placed over the right infraorbital foramen of the subject, and a large positive electrode, elliptical in shape (long axis, 11 cm., short axis, 5.5 cm.), was fitted on the right forearm. The electrodes were zinc, covered with gauze, soaked in saturated salt solution, and held in place by adhesive tape. By means of the introduction of a tungsten-filament vacuum tube in the stimulating circuit, it was possible to maintain the intensity of the shock at an appreciably constant value of 1.1 m.a., in-

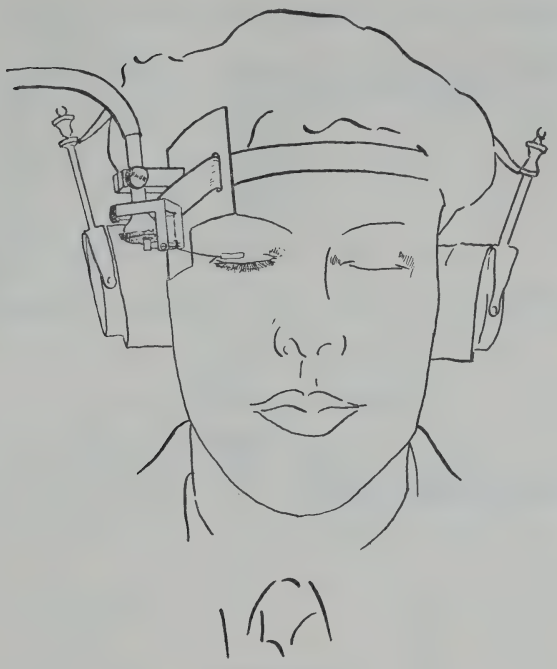

FIGURE 1
HEADGEAR OF SUBJECT

dependently of any variation in the resistance of the subject during the course of the experiment.[3]

[3]There were a number of factors operating to maintain the intensity of the current at an appreciably constant value independently of variations in the subject's resistance.

First, the inclusion of a 25,000-ohm series resistance in the circuit as a safety measure (see Figure 2 A) and the relatively high *minimum* resistance of the tube itself (c. 10,000 ohms) lessened the effect of variations of the subject's resistance upon the intensity of the current. It is obvious by Ohm's law that the higher the voltage and the greater the amount of constant resistance there is, the smaller will be the effect of variations of the subject's resistance upon the intensity of the current.

Another factor is the current-limiting properties of the saturated tungsten vacuum tube. If the plate potential is maintained at a value substantially above the knee of the plate current-plate potential curve, the intensity of the current will remain constant despite changes in the external resistance, if the changes in resistance do not lower the plate potential to a value ap-

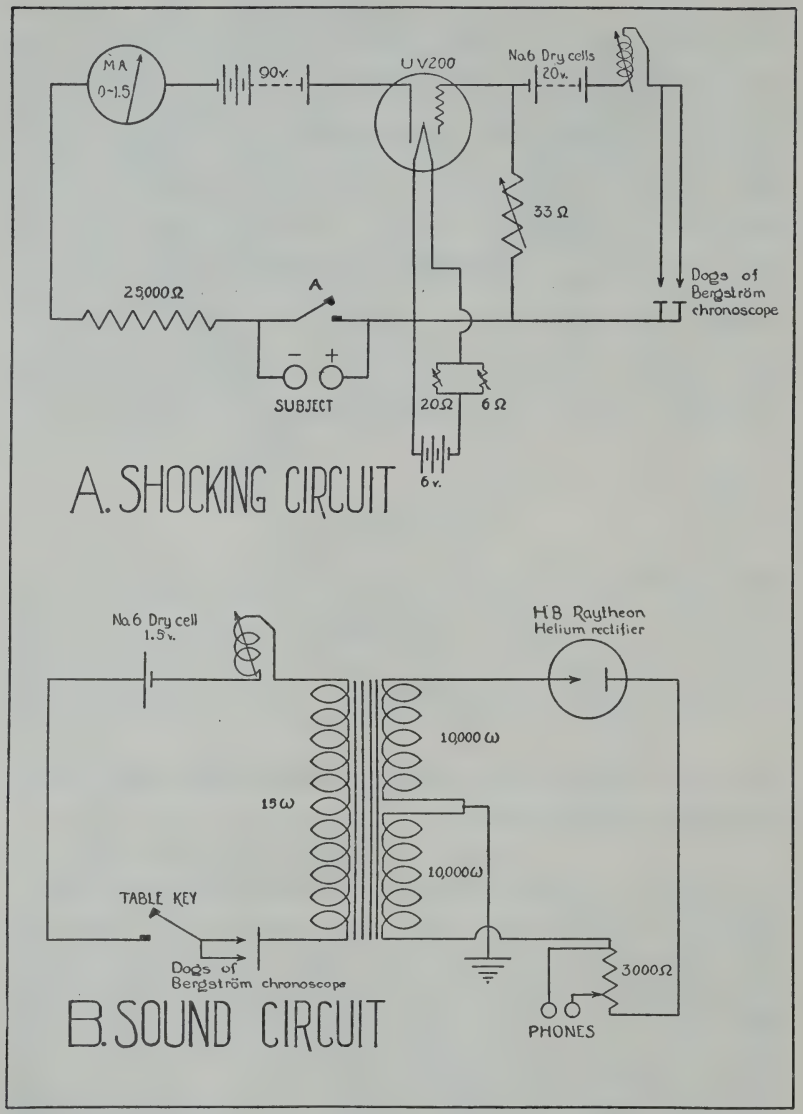

FIGURE 2
APPARATUS

The stimulating device which was employed is diagrammatically represented in Figure 2 A. The duration of the shock (34-38 sigma) was controlled by the arrangement of the break-dog and the make-dog on a Bergstrom chronoscope. The dogs made or broke a circuit which impressed a negative potential on the grid, thus controlling the flow of electrons from the filament to the plate of the vacuum tube. When the blocking circuit was made, there was no current passing through the circuit which included the subject. This was checked by the milliameter. The intensity of the current was controlled by regulating the temperature of the filament through the rheostats connected with the storage battery. The procedure was to short-circuit the subject by making the switch *A*. Then the current was set at 1.12 m.a. The short-circuit was then broken, allowing the current to flow through the subject. The intensity of the current usually dropped to 1.10 m.a. In a few cases the drop in intensity was slightly more than .02 m.a., in which cases the subject was again short-circuited, and the current intensity readjusted so that a current of 1.10 m.a. flowed when the subject was in the circuit. In other cases the drop in intensity was less than .02 m.a., and the intensity of the current was readjusted accordingly. It is evident that, if the introduction of the *total* resistance of the subject's body caused a decrease of .02 m.a. in the intensity of the current, variations in his resistance probably did not appreciably affect the intensity of the current. *This assumption was verified at the end of each experimental session.*

The time interval. The time interval between the conditioned

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proaching the knee of the curve. "It is seen that at high anode voltages the electron current is largely independent of anode voltage, being determined primarily by the cathode temperature . . ." (12, p. 102). The 90 volts impressed upon the circuit in the present set-up made the tube almost, but not quite, saturated. Near the saturation point, however, the change in the intensity of the plate current with respect to the plate potential is extremely small. (See curves in Terman *loc. cit.*) This explains why the introduction of the resistance of the subject in the circuit made such a small difference in the intensity of the current.

The change in the intensity of the current with respect to change in resistance under the conditions of the present set-up was determined by inserting standard metallic resistances of values of 1,000 to 18,000 ohms in the place of the subject in the circuit. It was found that the changes in the intensity of the current varied from .005 m.a. for 1,000 ohms to .035 m.a. for 18,000 ohms. There was a .020-m.a. drop in intensity for resistances of 8,000 to 10,000 ohms, and this indicates that the resistances of most of our subjects were within that range.

and the unconditioned stimulus was likewise controlled by means of the Bergstrom chronoscope. The arrangement of the dogs on the chronoscope (when the sound *preceded* the shock) was as follows: When the pendulum of the chronoscope was released, it first hit the break-dog of the sound circuit, causing a click in the headphones. After a fixed interval it hit the break-dog of the blocking circuit, allowing the current to flow through the subject; 34-38 sigma later it hit the make-dog of the blocking circuit, cutting short the flow of current. Then it hit the make-dog of the sound circuit. The dogs were then reset by hand—first, the break-dogs of both circuits, then the make-dogs. In this way no stimuli were presented during the resetting.

The conditioned stimulus. The click in the headphones which served as the conditioning stimulus was elicited by the set-up represented in Figure 2 B. The chronoscope pendulum, when it hit the first dog, broke the primary circuit, thus inducing a potential in the secondary circuit which discharged through the Raytheon helium tube and the headphones. When the second dog was struck, the primary circuit was made again, but no current flowed in the secondary circuit on account of the blocking action of the Raytheon helium rectifier. Thus extraneous stimuli in the form of secondary clicks were excluded.

The intensity of the click depended upon the voltage of the primary circuit, the source of which was a single dry cell. The voltage of the dry cell was constantly checked. Thus the voltage of the primary circuit was maintained at an appreciably constant value, and, therefore, the intensity of the click was appreciably constant.

The recording system. The recording system was pneumatic in nature. The subject wore a headgear which consisted of a rectangular sheet of aluminum, 11 cm. by 7 cm., which could be bent to fit the subject's head, and which was held in place by a leather strap (Figure 1). Attached to the aluminum sheet was a clamp carrying a tambour. A light but stiff piece of wire, 5 cm. long, was attached to the upper lid of the right eye and acted as a lever pivoted to a metal frame which was fastened on the cylinder of the tambour. The other end of the wire lever had a celluloid foot which was attached to the loose rubber membrane of the tambour. The cylinder of the tambour could be raised or lowered so that there was no difficulty in fitting the apparatus to different subjects. The headgear

was light (weight, 2.3 oz., excluding leather strap) and the subjects reported that it was not at all uncomfortable. The attachment of the wire to the lid was not perceived at all by the subjects. A piece of rubber tubing, 60 cm. long, connected the cylinder of the tambour with a "pneumodeik," recently described by Hudgins and Stetson (5). The pneumodeik recorded on a kymograph drum. The lag of this pneumatic system was determined and found to be approximately 10 sigma. In measuring the latent times of the reactions obtained, a correction of 10 sigma was made for this lag.

The kymograph drum was spun by hand to eliminate the noises connected with the stopping and the starting of the motor. The time interval, the duration of the shock, and the curves were checked by the appearance on the record of the vibrations of a 50-d.v. tuning fork. The pendulum of the chronoscope was also released by hand, making this a noiseless procedure. Signal magnets recorded the appearance of the stimuli on the record. A screen hid the experimenter and the apparatus from the subject.

Procedure. Each subject was tested on a single interval and not used again. With the exception of the 100-sigma interval, the sequence of stimuli was the following:[4]

1. *1-4 sound stimuli*—to determine the nature of the reaction to the sound.
2. *4-6 shock stimuli*—to secure norms of the unconditioned reaction for comparison with the conditioned reaction.
3. *50 paired stimuli.*
4. *5 or more sound stimuli*—an "experimental extinction" series—the conditioned reaction was considered to be extinguished when three successive presentations of the sound did not elicit it. In all cases, however, at least five sound stimuli were presented.
5. In some subjects, instructions to wink *voluntarily* to the sound stimulus.

The number of subjects for each interval varied from 10 to 18. An equal number of men and women were used for each interval.[5]

A single session lasted about 45 minutes. No effort was made to control rigidly the time interval between combinations of stimuli in

[4]See Section VI.

[5]With the exception of the .50-second interval in which there were 6 men and 5 women.

order to obviate any possibility of conditioning to that interval. The interval varied from 10 to 25 seconds. The subjects were instructed to fixate a card that was approximately 312 cm. in front of them and 71 cm. above them. The angle of elevation was therefore about 12 degrees. They were told that they were neither to inhibit winking nor to wink voluntarily, but merely to sit in the chair and make themselves as comfortable as possible. The breathing of the subjects was not controlled. Incidental winks were noted, but their appearance did not significantly interfere with the recording or with the interpretation of the records.[6]

Both the arrangement of the set-up and the behavior of the subjects indicated that no extraneous cues were presented. Controls (spinning of the drum, releasing of the pendulum) which were run during the experimental session did not elicit a wink or any other observable response.

III. Forms of Conditioned Eyelid Reaction

The study confirms the observations of recent investigators concerning the characteristics of conditioned reactions. The process is not a simple substitution of stimuli. The conditioned reaction is a new, an added, reaction to the sum total of reactions. The conditioned reactions had latent times (105-1210 sigma) of the same order as those of the voluntary reactions that were recorded (105-360 sigma).[7]

The complexity of the conditioning process is further indicated by the *varied forms* that the conditioned reaction took. A rough description of three types may be made. These types may be designated:

1. the "typical" conditioned reaction
2. the positive fixation
3. the negative fixation

[6]A few subjects with excessively high normal winking rates were rejected when it became apparent that these incidental winks might interfere with the interpretation of the records.

[7]The measures of the latent times and recruitment times of the reactions were obtained by a random sampling of the records of all the intervals. The extreme upper limit of the latent times of the conditioned reactions is due to the presence of a few conditioned reactions of long latency in the interval in which the sound preceded the shock by 1.48 seconds.

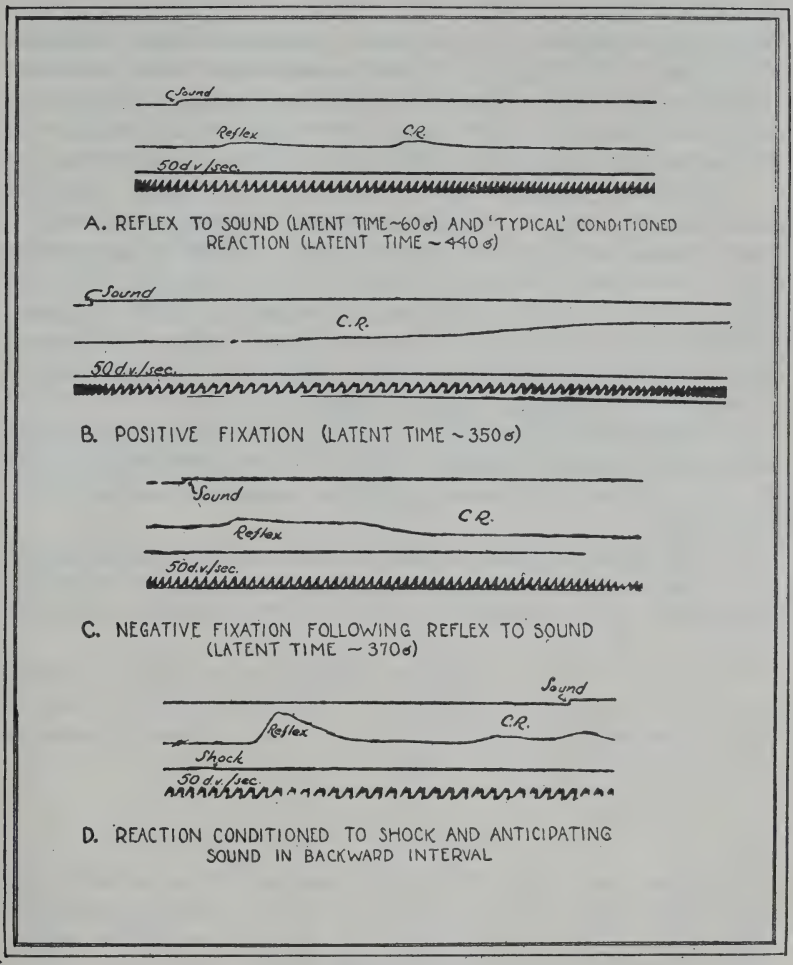

FIGURE 3

FORMS OF CONDITIONED EYELID REACTION

1. *The "typical" conditioned reaction* (*Figure 3 A*)—the conventional conditioned reaction resembling in its coarser aspects the unconditioned reactions to sound and shock, but differing in that its latency and recruitment time (rising phase of the reaction) are longer.

As has been stated above, the latent times were not less than those of the corresponding voluntary reactions (minimum latency recorded, 105 sigma). The latency of the unconditioned reflex to sound varied from 60 to 90 sigma and that of the unconditioned reflex to shock varied from 40 to 67 sigma. (See Figure III A and D for illustrations of unconditioned reflexes.) The recruitment times of the conditioned reactions varied from 50 to 95 sigma and were clearly slower than the recruitment times of the unconditioned reflexes to sound and to shock (20-40 sigma). The recruitment times of the corresponding voluntary reactions had a range of 70 to 95 sigma. This "typical" conditioned reaction resembled in general the type of response that Schlosberg (9) has described in his study of the conditioned knee-jerk, and that Hilgard (4) has described in his study of the conditioned eyelid reaction.

2. *The positive fixation* (*Figure 3 B*)—a tetanic *lowering* of the lids of long duration, presumably a fixation movement involving both the orbicularis and the levator, with the orbicularis component predominating.[8]

2. *The positive fixation* (*Figure 3 C*)—a tetanic *raising* of the lids of long duration which sometimes continues until the shock has been given. It is presumably a fixation of the orbicularis and the levator with the levator component predominating.

It should be understood that this description of "types" of conditioned eyelid reaction is in no sense a strict classification. Many of the reactions were not to be classed as any one of these types but as combinations or successions of them.

IV. The Unconditioned Reflex to Sound

Inhibition. In recent theoretical discussions of conditioning the generalization has been made that one of the essential phenomena in the conditioning of a reaction is the disappearance of the unconditioned reaction to the conditioned stimulus.[9]

The present set-up differs from the conventional experiments in conditioning in that the original reaction to the conditioning stimulus involves the same moving member and, in all probability, the same

[8]For a discussion of the distinction between the fixation type of movement and the ballistic "free thrown" movement, see Stetson and McDill (10).

[9]Cf. Borovski (1) and Razran (8).

muscle group as the reaction to the unconditioned stimulus. An examination of the data with respect to this hypothesis concerning decrement of the unconditioned reaction to the conditioning stimulus is, nevertheless, to the point to determine whether the reflex to sound is *negatively conditioned,* i.e., decreases in amplitude as a result of *paired* stimulation in contrast to mere repetition of the sound.

The sound stimulus frequently evoked a minimal lid-reaction of a latency of 60 to 90 sigma (Figure 3 A and C). The curves of nine subjects showing a relatively great amount of positive conditioning, as measured by the number of anticipatory reactions appearing in the 50 paired stimulations, were examined.[10] The amplitudes of the unconditioned reactions to the sound stimuli presented before, during, and after paired stimulation were measured to determine the nature of the changes taking place. The results for the individual subjects showing changes in actual amplitude and the averages for

TABLE 1

CHANGES IN AMPLITUDE IN MILLIMETERS OF UNCONDITIONED REFLEX TO SOUND IN NINE SUBJECTS, AND AVERAGE CHANGES IN AMPLITUDE IN TERMS OF PERCENTAGE FOR ALL THE SUBJECTS

Composite data for all subjects showing amplitude in terms of percentage.

Subject	Average amplitude of Ucr-So. before prd. stim.	Average amplitude 1-10 prd. stim.	Average amplitude 11-20 prd. stim.	21-30	31-40	41-50	Extinct. series
AF	0 cm.	.50	.49	.35	.15	.17	0
BC	.55	.63	.32	.45	.24	.19	.09
BF	0	1.07	.56	.46	.30	.52	.56
EA	0	.56	.51	.41	.15	.13	.24
EB	0	.15	.14	.25	.04	.05	0
CE	0	.08	.08	.30	.07	.10	.09
CG	1.00	.82	.62	.54	.45	.69	.33
CH	1.80	1.61	1.16	1.41	1.16	.77	.38
CO	0	.31	.42	.15	.48	.57	.20
	35.7 %	100 %	85.4 %	111.2%	57.5%	67.4%	38.9%

Note: *prd. stim.* = paired stimuli
extinct. series = extinction series

[10]At the stage of the investigation when these records were examined, they were the records of the nine subjects who had shown the *greatest* amount of conditioning at that date.

all the subjects in terms of percentage changes in amplitude are shown in Table 1.

1. Inspection of the data shows that decrement in the amplitude of the unconditioned reaction to sound is not an invariable accompaniment of the conditioning process. Six subjects (AF, BC, BF, EA, EB, CH) of the nine showed a decrease in amplitude during the period of paired stimulation. The reflexes of two subjects (CE, CG) remained approximately the same in amplitude, and the remaining subject (CO) showed an increase in amplitude.

2. In the case of four of the six subjects who showed decrement during the paired stimulation (AF, BF, EA, EB), there was no reaction to the sound when it was presented at the beginning of the experimental session. Evidently the isolated shocks which were presented after the sound stimuli (see Section II) had a facilitative effect on the sound-wink reaction system. *The decrement in these cases was merely an approach to the state of inexcitability that existed before any shocks were given.* The averaged data for all the subjects show this tendency very strikingly.

3. The corollary to the proposition of decrement during paired stimulation, namely, that *extinction* of the conditioned reaction involves facilitation of the unconditioned reaction to the conditioning stimulus (facilitation as compared with its amplitude when the conditioned reaction has been established), is not supported by the data. Four of the six subjects that showed decrement in the training series (AF, BC, EB, CH) showed further decrement of the unconditioned reaction to sound in the extinction series.

A control series of 30 sound stimuli alone were run on two of the subjects (AF, BF) who showed decrement in the training series. The reaction extinguished very rapidly (AF, last reaction on tenth stimulation; BF, last reaction on fourth stimulation). This indicates that the reaction to sound is actually facilitated during the training series.

In short, the data suggest that there is a facilitative effect during the training series that counteracts or delays the rapid disappearance of the response which would occur if the sound were repeatedly presented alone.

Facilitation. The question of whether this facilitative effect is a type of conditioning, i.e., a function of paired stimulation, is left unanswered by the data of the conditioning sessions. The hypothesis

that it is not actually the result of the *pairing* of stimuli, but a function of the sensitizing action of the shocks presented before paired stimulation is suggested by the fact that the facilitation is apparent at the very beginning of paired stimulation directly following the shocks. To test this idea the following sequence of stimuli was run on a number of subjects that had not been previously used:

1. 10 sound stimuli
2. 10 shock stimuli
3. 15 sound stimuli and 15 shock stimuli given in random order and *not paired*
4. 10 sound stimuli

If the subjects should show facilitation of the reflex to sound after the 10 shocks and during the random presentation of sounds and shocks, it would be clear that the facilitation observed during paired stimulation in the conditioning sessions was not due to the paired stimuli *qua* paired stimuli.

TABLE 2

CONTROL GROUP OF FOUR SUBJECTS SHOWING FACILITATION OF UNCONDITIONED REACTION TO SOUND WHEN SOUND AND SHOCK ARE *Not* PAIRED (IN MILLIMETERS)

Subject	1-5	6-10	1-5	6-10	1-5	6-10	11-15	1-5	6-10
	Before shock		*After* shock		*During* random presentation of sounds and shocks			*After* random presentation	
Cr	1.24	.24	2.96	1.96	2.46	4.04	2.90	1.54	1.40
Cl	.44	.00	.74	.12	.56	.46	.00	.00	.00
Ha	.40	2.00	4.72	1.60	3.20	3.70	2.20	.80	.90
St	1.04	.18	1.40	1.98	1.00	.32	.48	.34	.07

Note: *1-5, 6-10,* etc., equals mean amplitude of first 5 reactions, second 5 reactions, etc.

The results for the four subjects are tabulated (Table 2). The data for each of the subjects show clearly a facilitative tendency. Facilitation of the reflex always occurred after the 10 shock stimuli, and the increased amplitude of response was sustained during the random presentation of sounds and shocks in three of the four subjects (Cr, Cl, Ha). All three subjects who showed the facilitative effect during the random presentation of sounds and shocks showed rapid decrement in amplitude when the sound was again presented alone. The results show that facilitation of the reflex to sound does not develop

because the stimuli are *paired.* It is reasonable to conclude, therefore, that the facilitation observed during the conditioning sessions is merely a special case of this general sensitizing action of the shocks.

V. Time Interval and Amount of Conditioning

In securing the data on this question, nine time intervals were investigated, ranging from that in which the conditioned stimulus *preceded* the unconditioned stimulus by 1.48 seconds to that in which the conditioned stimulus *followed* the unconditioned stimulus by .90 seconds. The intervals were as follows: the sound preceding the shock by 1.48, 1.00, .50, .30, .25, .20, .10 seconds, and the sound following the shock by .50 and .90 seconds. At least ten subjects were used for each interval. The amount of conditioning in percentage as measured by the ratio of the number of conditioned reactions anticipating the unconditioned stimulus in the fifty paired stimulations to the number of paired stimulations and the stability of the conditioning as measured by the number of conditioned reactions appearing in the extinction series are shown for all the subjects in Table 3.

As the data show, there is great individual variability in performance, and none of the differences between the medians or means

TABLE 3
Percentage Conditioning and Stability of Conditioning in 101 Subjects
(See text)

	Series G Sound precedes shock by 1.48 second				*Series A* Sound precedes shock by 1.00 second		
Subj.	Sex	%C*	St†	Subj.	Sex	%C	St
GA	F	2	0	AA	M	2	0
GB	F	60	3	AB	M	12	0
GC	F	6	0	AC	M	44	5
GD	F	12	1	AD	F	34	2
GE	F	94	32	AE	F	24	0
GF	M	32	5	AF	F	80	24
GG	M	16	1	AG	M	6	0
GH	M	2	0	AH	F	20	1
GJ	M	44	15	AJ	M	40	3
GK	M	40	0	AK	F	0	0
Mean		30.8		Mean		26.2	
Median		24		Median		22	

TABLE 3 (*continued*)

Subj.	Sex	*Series B* Sound precedes shock by .50 second %C	St	Subj.	Sex	*Series E* Sound precedes shock by .30 second %C	St
BA	F	16	3	EA	F	76	5
BB	M	2	0	EB	F	58	1
BC	M	64	6	EC	F	54	7
BD	M	34	2	ED	M	18	1
BE	F	4	0	EE	M	8	0
BF	F	60	1	EF	M	0	0
BG	F	0	0	EG	F	44	10
BH	M	14	0	EH	F	40	1
BJ	F	18	?	EJ	F	20	0
BK	M	2	0	EK	M	4	0
BL	M	26	1	EL	M	48	1
				EM	M	8	0
Mean		21.8		Mean		31.5	
Median		16		Median		30	

Subj.	Sex	*Series C* Sound precedes shock by .25 second %C	St	Subj.	Sex	*Series D* Sound precedes shock by .20 second %C	St
CA	M	62	?	DA	M	18	1
CB	M	10	0	DB	F	0	0
CC	M	30	2	DC	M	0	0
CD	M	50	13	DD	M	6	0
CE	F	68	9	DE	F	0	0
CF	F	22	2	DF	M	6	0
CG	F	64	3	DG	F	76	18
CH	F	74	9	DH	F	0	0
CJ	M	0	0	DJ	M	0	0
CK	F	46	4	DK	F	26	6
CL	M	0	0	Mean		13.2	
CM	M	0	0	Median		3	
CN	F	8	1				
CO	M	70	3				
CP	M	12	0				
CQ	F	0	0				
CR	F	0	0				
CS	F	0	0				
Mean		28.7					
Median		17					

TABLE 3 (*continued*)

Series F Sound precedes shock by .10 second				*Series H* Sound follows shock by .50 second			
Subj.	Sex	%C	St	Subj.	Sex	%C	St
FA	F	2	0	HA	F	8	0
FB	M	0	0	HB	F	0	0
FC	M	0	0	HC	F	0	0
FD	M	0	0	HD	F	0	0
FE	M	0	0	HE	F	0	0
FF	M	4	0	HF	M	0	0
FG	F	0	0	HG	M	0	0
FH	F	4	0	HH	M	0	2
FJ	F	0	0	HJ	M	0	0
FK	F	0	0	HK	M	0	2
Mean		1.0		Mean		.8	
Median		0		Median		0	

Series J Sound follows shock by .90 second			
Subj.	Sex	%C	St
JA	M	0	0
JB	M	0	0
JC	M	4	0
JD	F	0	0
JE	F	2	1
JF	F	6	0
JG	F	0	0
JH	M	0	0
JJ	F	0	0
JK	M	4	0
Mean		1.6	
Median		0	

*%C = Percentage Conditioning
†St = Stability

of adjacent intervals is statistically significant. There is little difference in the ease of conditioning whether the sound precedes the shock by 1.48 seconds or by .30 seconds. At intervals less than .30 seconds the amount of conditioning becomes progressively less, so that at the .10-second interval there is practically no conditioning at all. In this respect, it is worth while to note that the difference between the central tendencies of the .30-second group and the .20-second group approaches statistical reliability. The chances in 100

that a true difference exists are 99 with respect to the medians and 96 with respect to the means.[11] Whatever be the process that affects the ease of conditioning as far as the time factor is concerned, it makes its appearance in the .20-.30-second intervals.

In the intervals in which the sound *followed* the shock, extremely little conditioning occurred. Only seven of the twenty subjects tested exhibited conditioned reactions, and the greatest amount of conditioning obtained in any subject was 8% (4 conditioned reactions).

The results are in substantial agreement with those of Hilgard (4) that the formation of the conditioned eyelid reaction is notably more difficult when the interval between stimuli is less than .20 seconds. The data obtained from the longer intervals do not support the hypothesis (13, 14) that the amount of conditioning in human subjects is a function of the time interval.

VI. A Consideration of Certain of the Intervals

The .10-second interval. From the data secured on the longer intervals (Section III), the generalization has been made that the latent times of conditioned reactions are of the order of those of voluntary reactions. This agrees with the findings of previous investigators (4, 9). It is evident that, if in the .10-second interval conditioned reactions of latencies of 140 sigma or longer appeared, they would not anticipate the unconditioned reflex to shock.[12] It might be argued, then, that our technique of discovering the conditioned reaction during paired stimulation fails at this interval and that no true measure of the amount of conditioning is obtained. There are two lines of evidence that militate against this argument:

1. The extinction series (the series of unreinforced sound stimuli presented at the end of paired stimulation) contains no conditioned reactions. The stability as measured by the number of conditioned reactions obtained is zero. In view of the high correlation to be found between the amount of conditioning and stability in the longer

[11]This difference is all the more significant since it is evident from an inspection of the data that a single extreme case in the .20-second interval (DG, 76% conditioning) has raised the mean of the group above what would probably be its representative value if a greater number of cases had been studied.

[12]100-sigma interval plus the 40-sigma latency of reflex to shock.

intervals (Section VII), it is extremely probable that there really is a lack of positive conditioning at this interval.

2. A second method of indirectly measuring the amount of conditioning is furnished by the observation of Hilgard (4, pp. 21-22) that, in the case of a single subject who had previously been conditioned at longer intervals, conditioning at intervals below 200 sigma consisted in facilitation of the reflex to the unconditioned stimulus.

To make a test to see whether this type of change occurred in the .10-second interval, the procedure was modified in the cases of eight of the ten subjects of this interval. The modified procedure was as follows:

1. 2-5 sound stimuli
2. 8-10 shock stimuli
3. 50 paired stimuli plus 3-5 isolated shock stimuli inserted at random during the series.
4. 5 or more sound stimuli
5. 8-10 shock stimuli

Thus, suitable control reactions to the shock alone which served as a basis for comparison with the reactions to the paired stimuli were obtained before, during, and after paired stimulation. The performances of the eight subjects, showing the average amplitude of reaction to shock before paired stimulation, the average amplitude when the shock was paired with the sound, and the average amplitude of reaction to the isolated shocks presented during and after paired stimulation are given in Table 4.

TABLE 4

THE AVERAGE EXTENT OF REACTION OF EIGHT SUBJECTS OF THE .10-SECOND INTERVAL TO ISOLATED SHOCKS PRESENTED BEFORE, DURING, AND AFTER PAIRED STIMULATION, AND TO PAIRED STIMULATION

Subject	*Before* paired stimuli	*To* paired stimuli	*After* paired stimuli	*Controls during* paired stimuli
FA	4.7 mm.	.8	none taken	2.9
FB	6.5	.4	2.9	2.8
FC	4.6	.3	6.7	12.9
FF	3.1	0	0	1.0
FG	5.9	3.8	4.0	3.5
FH	3.9	.8	4.3	2.4
FJ	3.0	.2	3.2	2.7
FK	7.5	4.8	5.2	6.5

In six of the eight subjects the amplitude of reaction to the shock when paired with the sound was markedly less than the amplitude of reaction to the isolated shocks presented before, during, and after paired stimulation. The remaining two subjects (FF, FG) showed no decrement that was specific to paired stimulation, although a general progressive decrement was evident. *In no case was there any evidence of facilitation resulting from the pairing of stimuli.*

Backward Conditioning. The observation that extremely little conditioning occurred either during paired stimulation or during the extinction series in the backward intervals does not agree very closely with the results of Switzer (11), who seems to have secured backward conditioning of eyelid reactions much more easily. In Switzer's experiment 18 out of 20 subjects showed conditioned reactions, while in this experiment only 7 out of 20 subjects were conditioned.[13] One fact suggests itself as a possible explanation of the discrepancy between the results of Switzer's investigation and the results of the present one. In the discussion of the unconditioned reaction to sound (Section IV), it was shown that, while facilitation of the lid-reflex to the sound sometimes occurred during the conditioning session, such facilitation is not due to the pairing of stimuli and, therefore, cannot be considered as a type of conditioning.[14]

The same facilitation was observed in eight of the twenty subjects tested in the backward intervals. Of these eight, six did not respond to the sound when it was presented at the beginning of the session. Of the remaining two subjects, one responded to the first presentation of the sound, but not to the second, thus satisfying Switzer's criterion in selecting subjects.[15] If the latencies of these facilitated reflexes were not measurable they might have been considered conditioned reactions. Switzer did not measure the latent times of the reported conditioned reactions of his subjects, and it is

[13]Of the 18 subjects conditioned by Switzer, 16 exhibited conditioned reactions before the 50th paired presentation so that the limited number of paired presentations in the present experiment is not a significant factor in a consideration of the difference in the results of the two experiments.

[14]The facilitation may be a function of the past history of the subject and thus may be considered as "conditioning," but it is *not* the result of this specific conditioning set-up (pairing of sound and shock), and that is the point with which we are here concerned.

[15]"An individual was considered unsuitable if the buzzer caused him to wink twice in succession when it was sounded at the beginning of the experiment" (11, pp. 80-81).

possible that some of these were not conditioned reactions, but facilitated unconditioned reactions.[16] That the buzz which Switzer used as the conditioning stimulus can be an adequate stimulus for the lid-reflex is indicated by the fact that some of his subjects were rejected because they winked to the buzz at the beginning of the experiment. (See footnote 15.)

VII. Amount of Conditioning and Stability

The correlation between the amount of conditioning as measured by the number of anticipatory reactions appearing in the 50 paired stimulations and the stability of such conditioning as measured by the number of conditioned reactions obtained in the extinction series were examined for a group of 69 subjects.[17] A correlation coefficient of .88 was obtained (method of rank differences). The P.E. was .02.

VIII. The Conditioned Reaction to Shock

In the intervals in which the sound followed the shock there appeared a new phenomenon, *reactions conditioned to shock* and anticipating the sound (Figure 3 D). Ten of the 20 subjects used in the backward intervals made such conditioned reactions to the shock.

The performances of the 20 subjects with respect to these conditioned reactions to shock is given in Table 5, which shows the ratio in percentage of the number of such conditioned reactions to the 50 paired stimulations.

The fact that conditioned reactions to the stronger of the two stimuli (shock) were formed casts doubt upon the validity of the hypothesis that conditioning always takes place to the weaker of two stimuli, never to the stronger.[18] Conditioned reactions can be

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[16]"Unconditioned" with respect to the conditioning set-up. See footnote 14.

[17]The data of the .10-second interval were not considered since the correlation found between the measures of the longer intervals is cited to confirm the conclusion that no positive conditioning occurred at that interval. The data of the backward intervals were not considered since the arrangement of the stimuli (sound following shock) precludes the possibility of a reaction to sound anticipating the reaction to shock. Of the 71 subjects run in the other intervals, two (BJ, CA) could not be considered because extinction series had not been run on them.

[18]Cf. Pavlov (6, pp. 30-31), Borovski (1, p. 6)—"prepotency rule," Razran (8).

TABLE 5
THE PERCENTAGE OF CONDITIONING TO SHOCK IN THE SUBJECTS USED IN THE BACKWARD INTERVALS

Shock *.50 sec.* Sound		Shock *.90 sec.* Sound	
HA	0	JA	2
HB	0	JB	0
HC	0	JC	0
HD	0	JD	6
HE	2	JE	20
HF	2	JF	48
HG	8	JG	0
HH	30	JH	0
HJ	46	JJ	0
HK	2	JK	0
Mean	9.0	Mean	7.6

formed to the stronger of two stimuli. Nevertheless, that the relative strength of stimuli is a significant factor in the formation of the conditioned reaction can be readily seen by comparing the amount of conditioning to the *shock* when the shock preceded the sound by .50 and .90 seconds (Table 5), with the amount of conditioning to the *sound* when the sound preceded the shock by .50 and 1.00 seconds (Table 3). We have here a comparison under practically identical time relations. Conditioned reactions are much more readily established to the sound than to the shock. In the sound-preceding-shock intervals, 10 of the 21 subjects made 10 or more conditioned reactions to the sound in the 50 paired stimulations. In the shock-preceding-sound intervals only 4 of the 20 subjects made 10 or more conditioned reactions to the shock. The mean percentage of conditioning of the 21 subjects to the sound in the sound-preceding-shock intervals is 24. The mean percentage of conditioning of the 20 subjects to the shock in the shock-preceding-sound intervals is 8.3. The difference in the mean percentages of conditioning can hardly be considered a quantitative measure of the extent to which relative strength of stimuli is a factor in determining the amount of conditioning, but it clearly shows that conditioning to the sound is more readily established than conditioning to the shock.

The occurrence of both *forward* conditioning to the shock and *backward* conditioning to the sound in the shock-preceding-sound intervals makes it possible to compare the importance of the precedence in time of either of the stimuli (the question of which stimulus is

first presented) with the factor of relative strength of stimuli with respect to their significance in determining the amount of conditioning. It is clear that there is more conditioning to the shock. No subject made more than 4 conditioned reactions to the sound. Thirteen of the 20 subjects made no conditioned reactions to the sound. The mean percentage of backward conditioning to the sound is 1.2. This may be compared with the 8.3% of forward conditioning to the shock. There is every indication that forward conditioning to the shock is more readily established than backward conditioning to the sound, and on this basis it may be concluded that the factor of temporal precedence is more significant in determining the amount of conditioning than the factor of relative strength of stimuli.

IX. Summary

A study of the formation of conditioned eyelid reactions in human subjects yields the following results:

1. Conditioned eyelid reactions occur in a variety of forms, ranging from those which resemble in its coarser aspects the unconditioned lid-reflex to those which consist of tetanic raising and lowering of the lid. A common feature of all these forms is the latent time which is of the order of the latent times of voluntary reactions.

2. Facilitation of the unconditioned reaction to the conditioning stimulus (lid-reflex to sound) frequently occurs during the conditioning session, but such facilitation is not due to the *pairing* of the specific stimuli employed in the conditioning experiment.

3. When the conditioned stimulus (sound) precedes the unconditioned stimulus (shock) by a range of .30 second to 1.48 seconds there is no variation in the amount of conditioning. When the conditioned stimulus precedes the unconditioned stimulus by less than .30 second, the amount of conditioning becomes progressively less so that at the .10-second interval there is practically no conditioning at all.

4. When the conditioned stimulus (sound) *follows* the unconditioned stimulus (shock) extremely little conditioning occurs.

5. A high correlation exists between the amount of conditioning as measured by the number of conditioned reactions occurring in a series of paired presentations of the conditioned and unconditioned stimuli and the stability of such conditioning as measured by the

number of conditioned reactions occurring in a series of unreinforced conditioned stimuli.

6. When the conditioned stimulus (sound) follows the unconditioned stimulus (shock), conditioning to the shock may occur. The amount of such conditioning to the shock is less than the amount of conditioning to the sound when the sound precedes the shock by the same intervals, and this difference indicates that relative strength of stimuli is a significant factor in the formation of the conditioned reaction.

7. When the conditioned stimulus (sound) follows the unconditioned stimulus (shock), the amount of conditioning to the shock is greater than the amount of conditioning to the sound (the amount of backward conditioning). This indicates that the factor of precedence in time of either stimulus is more significant in determining the amount of conditioning than the factor of relative strength of stimulus.

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FACTEURS DE TEMPS DANS LA FORMATION DES RÉACTIONS CONDITIONNELLES DES PAUPIÈRES CHEZ LES SUJETS HUMAINS

(Résumé)

On a fait une étude de la formation des réactions conditionnelles des paupières chez 101 sujets humains à l'égard de l'influence de certains facteurs de temps sur la quantité et le caractère du conditionnement.

Les résultats indiquent que:

1—quand le stimulus conditionnel (son) précède le stimulus non conditionnel (choc) dans une variation de 0,30 secondes à 1,48 secondes il n'y a aucune variation dans la quantité du conditionnement. Quand le stimulus conditionnel précède le stimulus non conditionnel de moins de 0,30 secondes la quantité du conditionnement devient progressivement plus petite de sorte qu'à l'intervalle de 0,10 d'une seconde il n'y a pratiquement nul conditionnement.

2—quand le stimulus conditionnel (son) suit le stimulus non conditionnel (choc) il se montre très peu de conditionnement au son.

3—quand le stimulus conditionnel (son) suit le stimulus non conditionnel (choc) le conditionnement au choc peut se montrer. La quantité de ce conditionnement *avancé* au choc est plus grande que la quantité du conditionnement *en arrière* au son chez les mêmes sujets. Cela indique que le facteur de précédence de temps de l'un ou de l'autre stimulus est plus signifiant pour la détermination de la quantité du conditionnement que le facteur de la force relative du stimulus.

Bernstein

ZEITLICHE FAKTOREN IN DER BILDUNG BEDINGTER AUGENLIDREAKTIONEN BEI MENSCHEN

(Referat)

Die Untersuchung betrifft die Bildung bedingter Augenlidreaktionen bei 101 Versuchspersonen hinsichtlich des Einflusses gewisser zeitlicher Faktoren auf den Umfang und den Charakter der Erstellung einer bedingten Reaktion.

Die Ergebnisse zeigen:

1. Wenn der bedingende Reiz (Ton) dem unbedingten (Choc) im Unfange von 0,30 bis 1,48 Sekunden vorangeht, so gibt es keine Variation im Umfang der Erstellung einer bedingten Reaktion. Wenn der bedingende Reiz dem unbedingten Reiz um weniger als 0,30 Sekunden vorangeht, so verringert sich der Umfang der Reaktionsbedingung progressiv, sodass beim 0,10—Sekunden—Intervall fast gar keine Reaktionsbedingung vorhanden ist.

2. Wenn der bedingende Reiz (Ton) dem unbedingten (Choc) folgt, so findet äusserst wenig Reaktionsbedingung auf den Ton statt.

3. Wenn der bedingende Reiz (Ton) dem unbedingten (Choc) folgt, kann Reaktionsbedingung auf den Choc stattfinden. Der Umfang solcher *Vorwärts*bedingung auf den Choc ist grösser als der Umfang der *Rückwärts*bedingung auf den Ton bei derselben Versuchsperson. Dies zeigt, dass der Faktor des zeitlichen Vorangehens auf irgend einen des Reize bedeutungsvoller ist in der Bestimmung des Umfangs der Bedingung als der Faktor der relativen Reizstärke.

BERNSTEIN

SHORT ARTICLES AND NOTES

VOLUNTARY ACTIVITY FROM THE STANDPOINT OF BEHAVIORISM*

WALTER S. HUNTER AND CLARENCE V. HUDGINS

In the present paper I wish to develop the theoretical conception of voluntary action and also to offer a concrete illustration of the behavioristic point of view in terms of an experiment performed under my direction at Clark University by Dr. Clarence Hudgins on the acquisition of voluntary control over the pupillary response (2). This experiment not only makes important factual contributions to our understanding of behavior, but it exemplifies in a clear-cut fashion the behavioristic mode of attack upon one of the fundamental problems of psychology.

When one speaks of behaviorism, many psychologists are still inclined to think of a radical system defended by a few American investigators, a system which should be grouped historically with such others as functionalism and structuralism. From my point of view nothing could be further from the truth. Behaviorism is not a system. It is the science of behavior; and its adherents include all who carefully and objectively seek to determine the nature and explanation of such phenomena. All behavior is not included in the subject-matter of this science. Much of it belongs to the science of biology. *Behaviorism specializes in the study of the extrinsic behavior of the organism to an external environment which is predominantly social in character.* I have elaborated this point of view elsewhere (3), and in the present connection I need only comment upon the significance of the term "extrinsic behavior." This can best be done by contrasting extrinsic and intrinsic responses. Intrinsic behavior is the peculiar function of a given organ. Thus digestion is the function of the alimentary tract; secretion is the function of the glands; and grasping and locomotion are the functions of the hands and feet. These intrinsic functions are the fundamental and typical subject-matter of physiology. In the psychology of behavior, however, we are interested in such responses as typewriting, tapping, drawing, and language. None of these activities is a peculiar function of any organ of the body. These extrinsic forms

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of behavior, although normally carried out by certain muscle groups, need not be so performed. If the muscles concerned are lost or rendered inactive, the behavior occurs in other effectors. In contrast with this situation is that offered by intrinsic responses which can be performed only by such specific organs as the heart, the hand, or the lungs. Behaviorism is not the science of the organism as a whole as contrasted with physiology, the science of the behavior of parts of the organism. Such a distinction is invalid because all behavior is to some degree the behavior of the organism as a whole. Although the pupillary responses which we are to discuss are intrinsic functions of the eye, the problem with which we are concerned is well over into the field of psychology, since we are concerned with certain broad characteristics of behavior control and use the iris mechanism only as a particularly convenient subject for the solution of this problem.

It is hardly to be expected that an objective, experimental science like behaviorism should take an interest in the general problem of the will, for the will has been defined as one of the aspects of mental life, the aspect of conation, of purposive striving, and of decision. This theoretical concept dates from the time of the Greeks. It has played no great rôle in experimental psychology, however, because careful experimenters have been unable to isolate it for study. Even classical introspection has never revealed a "conscious quale" which might be termed volition. Experiments have been made with some success on the problem of the acquisition and control of the behavior called voluntary, as illustrated in Ach's study on *Willenstätigkeit und das Denken,* Bair's work on the *Control of Voluntary Movement of the Ear,* and Woodworth's investigation of the voluntary control of the movement of the large toe. It is difficult, however, to secure from these writers, or from others, a list of the criteria by which behavior can be classified into voluntary and non-voluntary groups.

Let us for the moment consider the question of the criteria of voluntary action. The striated muscles are often termed the voluntary muscles, while the smooth muscles by contrast are often called involuntary. From the traditional point of view such a division has some justification, inasmuch as practically all voluntary movements are made with the striated muscles, and inasmuch as the individual does not normally secure a voluntary control over the smooth-muscle groups. The distinction, however, is only a rough one because many of the responses of striated muscle are not voluntary but reflex and automatic, and because some voluntary control of smooth-muscle activity has been found. The problem of why, under the normal conditions of life, so-called voluntary activity is so largely confined to the striated muscles is one which should receive a careful analysis.

If there are distinguishing criteria of voluntary behavior they will probably be found to lie in one or all of three places: (1) in the history of

the response; (2) in the type of control to which the response is subjected; and (3) in such measures of reaction as latency and form of response. Let us consider these three possibilities in the order mentioned.

In the first place, all activity that might be called voluntary has in it an essential element of the acquired. This is clearly true in such behavior as writing and maze running. The generalization is probably justified that any habit which is well retained can be voluntarily initiated. Indeed, such voluntary reinstatement is the so-called method of recall used in the study of memory. Where there is voluntary control of such unlearned behavior as the reflex of winking or breathing, it would seem quite clear that the voluntary aspect is acquired.

In the second place, to call an act voluntary is to imply some specific type of control of this activity. As careful experimentalists, we cannot accept the hypothesis that the control is mental. To be sure, we cannot disprove such a view any more than we can disprove the hypothesis that mental forces are involved in the action of electricity and gravitation; but neither view has any scientific validity because neither is susceptible to experimental check. Some psychologists would tell us that voluntary acts are controlled by purposes or intentions either in the form of determining tendencies or of sensations and images of the movements to be executed. Voluntary activity is thus for them not to be distinguished from purposive behavior. Now the terms "purpose" and "intention" fulfill their functions in everyday discourse, and they may be valuable for certain qualitative descriptions, but experience has failed to justify their use in rigorous experimentation. There is no need to make a mystery of the control and regulation of any case of behavior, although the complexity of the problem should not be minimized. It is an excellent hypothesis that all behavior is controlled by stimuli and by the receptor-neural processes thereby set up. Certainly this theory, because of its genetic simplicity and its susceptibility to experimental test, should be the basis of departure for all attempts at explaining the nature of the control of behavior.

The particular stimuli most significantly involved in the control of voluntary movement are those generated by the behavior of the organism itself. The kinaesthetic, tactual, and auditory stimuli involved in language are the most important self-induced stimuli in man (4, p. 272). By the aid of such receptor processes the organism becomes relatively independent of its external environment and can regulate its own behavior to an extent impossible in the infrahuman animals. Behavior controlled by the organism's own language responses is voluntary in the highest degree.

In the third place, we suggested that it might be possible to identify voluntary action by means of peculiarities in the latency and form of the response. In support of this suggestion we may point to the work of Schlosberg (5), Wendt (6), and Hilgard (1), among others, who have found that

the latency for certain conditioned responses is comparable with that for voluntary movement of the same organ, the latency in each case being greater than for the corresponding reflex. The results secured by Schlosberg and Wendt indicate further that the temporal form of response is the same for corresponding voluntary and conditioned responses, while the reaction when made reflexly has quite another form. These investigators do not tell us how to distinguish between a voluntary and a conditioned response, but the suggestion from our second criterion above is clearly that voluntary action is not only conditioned action, but action under the control of the self-excited receptors.

Let us now turn to a consideration of Dr. Hudgins' experiment. The problem was to condition the pupillary reflex of the human eye to such self-excited stimuli as words and the tactual-kinaesthetic processes arising from the contractions of the subject's hand and arm muscles. If the pupil could be so conditioned, the response of contraction or dilation could be aroused whenever the subject through his behavior reinstated the proper stimuli. Since this would be acquired, self-regulated behavior occurring relatively independently of external conditions, behavior which furthermore would be under the control of the subject's language responses, it would have at least the first two of the objectively verifiable characteristics of voluntary activity. An examination of the latency and form of response might also show that verbally controlled pupillary responses were significantly different in these respects from the unconditioned pupillary responses. Furthermore, since most of the subjects were in ignorance of the fact that their iris muscles were being trained, there could have been present no "intention" or "will" to contract or dilate the pupil. Inasmuch as the iris is not supplied with receptors, no direct stimulation could result from its activities. Thus, if an objectively verifiable voluntary control of the pupillary response could be established, this control could be exercised neither by stimuli from the responding organ nor by the subject's purposes and intentions to move the iris.

The general method used in conditioning the pupil was that followed by Cason, and an apparatus similar to his was employed. This apparatus was so arranged that a light could be directed into the subject's eye while at the same time the experimenter could measure the diameter of the pupil by means of adjustable parallel hairs in the ocular of a telescope (magnification, 5 times). In certain of the experiments arrangements were made to secure kymograph records of the temporal course of the response by connecting the micrometer screw of the telescope to a writing lever which recorded on the smoked paper. These graphic records made possible the study of the latency and form of the response. The following stimuli were used: a training light of 37.7 candle meters, a test light of 3.5 candle meters, the sound of a bell, tactual-kinaesthetic stimuli aroused in con-

tracting the hand when the subject closed a dynamometer switch, and, finally, auditory verbal stimuli. These latter might be given by the experimenter or by the subject, and they might be either nonsense words or the words "contract" and "relax." During the training and testing the subject fixated two crossed wires which were superimposed on the stimulus area.

Fourteen subjects were used, of whom only four knew that the experiment concerned the conditioning of the pupillary response.

The general procedure and results of the experiment were as follows: (1) The effect was determined of the bell, the hand contractions, and the verbal stimuli when each was presented simultaneously with the test light prior to any training. In every case prior to training these stimuli produced a dilation of the pupil. (2) Training the pupil was now begun by presenting the training light and the bell simultaneously. After from 100 to 200 presentations of the bell, the pupil was so conditioned that, when the bell stimulus was added to that of the test light, the pupil would contract, whereas prior to the training it would dilate. This is in strict accordance with the results of Cason. (3) When this stage of the training had been reached, the subject was instructed to place his hand on the dynamometer and to contract or relax this hand as the experimenter commanded. There now followed a training period during which the training light, the bell, verbal commands "contract" and "relax," and the hand responses were all present. As a result of this training it was found that the pupil would dilate or contract when the subject's hand contracted or relaxed at the experimenter's command, although the bell stimulus was absent during the tests. (4) Two hundred more training trials were now given, using the light, the hand responses, and the verbal commands, but not using the bell. The subject was instructed to repeat subvocally the words "contract" and "relax" whenever the experimenter said these words. After this training, tests revealed that the pupil would contract or dilate when the subject said aloud the words "contract" or "relax," respectively. This would also happen when the subject whispered the words, or when he repeated them subvocally. In other words, the subject now had apparently the same type of control over the activity of the iris that he had over the activity of his hand. His hand would contract or relax at his command, and so would his iris muscles. Since the subject did not know the purpose of the experiment, and since no afferent impulses were directly set up by the iris movements, it was impossible for the control of this voluntary activity to be dominated either by intention or by cues from the movement itself. *We are therefore led to believe that the voluntary control of movement is exercised by those stimuli under the control of the subject which have been effectively associated with the movement in question by means, in the present case, of the conditioned-response technique.*

Certain of the control experiments which were made should be described

here. (1) Since accommodation-convergence reactions of the eye will produce changes in the pupillary diameter, and since absolute fixation by the eye is impossible, it might be held that the conditioned responses described above were merely responses to these eye movements. The regularity of the conditioned responses as compared with the irregularity of the uncontrolled eye movements makes this hypothesis unlikely. Nevertheless, a control was made in which the pupillary changes were measured when the fixation was shifted from the regular fixation point to one 116 cm. nearer the eye. The eye movement which resulted was certainly greater than any irregular movement during the experiment, and yet the results show that the consequent change in diameter of the pupil was significantly less than that secured by the subject's use of verbal stimuli. It would seem reasonable, therefore, to conclude that the irregular eye movements which were present during the experiments on conditioning were insufficient to produce the results secured. (2) Controls were also made to determine whether or not the meaning, i.e., the past history, of the words "contract" and "relax" was vitally involved in the production of the pupillary responses. The conclusion is that such old meanings were of no consequence. In the first place, both words produced dilation of the pupil prior to training, whereas after training the word "contract" produced a contraction and the word "relax" produced a still greater dilation of the pupil. In the second place, some subjects were trained with the verbal stimulus "contract" paired with darkness and the stimulus "relax" paired with light. (This is the reverse of the normal procedure.) As a result, the stimulus word "contract" caused a dilation of the pupil, and the stimulus word "relax" produced a contraction of the pupil. It is therefore clear that the verbal stimuli acquired their control of the iris muscles solely as the result of the training given during the experiment. In the third place, some subjects were trained with nonsense words instead of the usual words "contract" and "relax." Except for this change, all conditions were the same as described above. The results showed that the nonsense words spoken, whispered, or repeated subvocally would arouse the appropriate responses.

The latency of the pupillary light reflex is given by authorities as from .2 to .5 sec. The average latency of the conditioned dilation response, however, Hudgins determined to be 1.56 sec., and of the conditioned contraction response, 2.29 sec. The duration of the light reflex is given as from 1 to 4 sec., with the initial contraction occurring very rapidly. Hudgins found the average response time for the conditioned dilation response to be 8.24 sec. and for the conditioned contraction response to be 10.83 sec. (The differences between the above pairs of averages are statistically significant.) Furthermore, the conditioned response was a more gradual response initially than was the light reflex. There were apparently no differences in these measures between the responses to the bell and to the verbal stimuli. The

results on latency and form of response are thus in harmony with those secured by other investigators who found marked differences between unconditioned and conditioned responses, but none between conditioned and voluntary ones.

In conclusion, we are of the opinion that the attempted classification of behavior into voluntary and non-voluntary forms of response is of less significance than a classification of the corresponding behavior in terms of genesis, type of control, and temporal characteristics. An adequate account of what the psychologist has called voluntary action cannot as yet be given because the necessary experimental data have not yet been secured; but the hypothesis is offered that so-called voluntary behavior is essentially a conditioned response having a characteristic latency and temporal course and under the control of self-excited receptor processes.

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AGE AND THE KINEPHANTOM*

Walter R. Miles

The imaginative activity of adults or their ability to organize totality meanings from sensory experience or the memory traces of experience has often been studied through the use of ink-blots, the Rorschach plates, figure-and-ground patterns, reversible perspective, mental images of objects, the overt creation of designs or forms, the writing of fantastic stories, and so on. The normal, intelligent individual who is examined by one of these media or techniques realizes quite well that it is he himself who arranges the materal into meaningful form and that he selects and emphasizes

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certain elements. He is fully aware that the ink-blot does not change its contour. It now "looks like a tree" and later "like a face" because he "sees" these things in it.

The present investigation has been carried out in such a manner that subjective interpretations were assumed by the subjects to be purely objective sequential happenings in a visual stimulus field. The instruction was, "Describe what you see taking place." The stimulus was a symmetrically moving silhouette formed by projecting the shadow of a two-bladed fan (cardboard rectangle 2 x 10 cm.) revolved one revolution per second in a plane at right angles to the translucent screen. This device for projecting moving shadows which easily lend themselves to various interpretations of movement pattern I have called a kinephantoscope. The visual experiences of the movement that do not correspond with the "physical reality" (revolving clockwise) I call kinephantoms. These effects belong to the general field of reversible perspective phenomena and are unique chiefly by reason of the dynamic character of the stimulus and its interpretations.

The windmill illusion (2) historically represents the beginnings of the present enquiry, and in a previous paper (1) I have traced the development of the kinephantom experiment to my observations of the windmill illusion. The observer typically describes the windmill as revolving in one direction or the other. But in my study I found that a 2-bladed mill, which I now call the kinephantoscope, seen as a silhouette gives the additional interpretations of "stretching," "clapping," "oscillating," and "crossing" as well as miscellaneous other meanings. I have now a more complete list of the meanings that may be seen or created from this type of stimulus as follows:

I. Revolving group; movement in two planes:

0. Turning right, clockwise movement as viewed from the top. (Not a kinephantom as this is the physically correct description.)

1. Turning left, counterclockwise.

2. Oscillating symmetrically, like a weathercock or vane, without a complete revolution in either direction.

3. Oscillating at an angle, i.e., not on a line corresponding with the line of sight.

4. One part revolving while the other moves in and out radially.

II. Hinge group; movement in two planes:

1. Arms clapping in front; symmetrically and with 90° action of each straight arm.

2. Arms clapping in back.

3. Clapping alternately front and back.

4. Clapping at an angle, i.e., not on a line corresponding with the line of sight.

5. "Clapping through" or passing through; each arm swinging 180° and in opposite directions, thus they pass through each other.

6. Clapping against the post; dislocation of the axis of the hinge to the front or back. The hinge has no apparent support.

7. Curved arms banging together.

III. Stretching group; movement in one plane:

1. Accordion-like, spreading and shrinking, out and in, "standing still."

2. Shuttling; two ends exchanging positions.

3. Shutter appearance of the shadow, it opens and closes itself like a camera shutter.

4. Shutter appearance of the frame around the shadow; the white flows over the dark and then uncovers it again.

5. Rolling up and then unrolling from both ends as a scroll.

Doubtless other perfectly workable meanings will be found as observations accumulate. Not all of these kinephantoms are equally likely to occur from a given stimulus or in the responses from a particular observer. The matter of relative incidence and persistence and of sequence must be reserved for later discussion. Incidentally, it may be mentioned as a noteworthy fact that kinephantoms "perform" as symmetrical totalities, each one in its turn involving the complete dynamic stimulus. In only one instance did an observer report that the two halves were executing different types of movement (see I-4 above).

Kinephantom data were secured as a part of the Stanford Later Maturity Study conducted under my direction in Palo Alto, California, in 1932. The subjects reporting were adults ranging in age from 25 to 90, and included 616 males and 587 females. Each subject was examined individually and each reported verbally or by means of gestures what he or she saw happening on the kinephantoscope screen during a period of 3 minutes. The "report of occurrence" nature of the procedure was given verisimilitude by the position of the examiner where he could not see the screen. He recorded the descriptions and noted each time a change in type of motion was mentioned.

There were 15 subjects (10 men and 5 women) who reported nothing but continuous "revolving clockwise" during the entire period of observation, i.e., they saw no kinephantoms. Fourteen subjects (2 men and 12 women) saw as many as 5 different kinephantoms and there was one person, a woman, who saw six. The average adult reported 2 or 3. There appears to be a rather distinct sex difference as can be seen in the following percentages:

No. of kinephantoms	0	1	2	3	4	5	6	7
Men, per cent	1.6	32.3	41.4	19.5	4.9	0.3	0	0
Women, per cent	0.9	8.9	35.6	39.8	12.8	2.0	0.2	0

Similarly it was found that the men reported fewer phases or changes within the standard 3-minute period than did the women. The averages were about 20 and 26, respectively.

When classified in terms of age the data reveal the rather surprising fact that young, middle-aged, and old people give practically identical results as to both aspects: the number of kinephantoms and the number of phases. Stated in four age groups the combined data show the following comparisons:

Age groups	25-29	30-49	50-69	70-89
Average no. of kinephantoms:	2.42	2.39	2.31	2.22
Average no. of phases:	24.0	23.0	22.2	23.9

There appears in this table a very slight fall in number of meanings and in number of phases from young adulthood to old age but the total change in each measure does not amount to 10 per cent for the mixed population. For men the change in number of reported types of movement is negligible, and there is some increase in phases with age, particularly in later maturity; on the other hand, women reveal a slow progressive decrease in number of interpretations and also a decrease in the number of reported phases which in the measures for the sexes combined more than overbalances the tendencies shown by the men. Other factors, such as education and intelligence, appear to have even a slighter relationship to the kinephantom results than do sex and age.

In conclusion, it may be said that creative imagination measured as a spontaneous activity, occurring with or incidental to the visual perception of a moving shadow stimulus and without any given or implied instructions directing the observer's attention to the possibility of subjective modifications, reveals psychological behavior of a characteristic kind and with a variety and persistence almost independent of age, intelligence, and education, but not of sex. To what extent the kinephantoms are to be explained in terms of experience, personality type, or some other category may become clearer with further experimentation. I believe that through dynamic reversible perspective phenomena, which may have either objective or subjective reference, a new approach is made to the mental aspects of personality.

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INFLUENCE OF SPEED AND AGE ON INTELLIGENCE SCORES OF ADULTS*

CATHARINE COX MILES

The gradual rise of intelligence test scores from the earliest testable age to a maximum somewhere in the teens is well established. High positive correlation between these test scores and chronological age in populations of normal children and youths is regularly expected. Little information relating age and intelligence test scores has, however, been available for the period of adulthood, although many intelligence tests furnish so-called "adult norms" on the basis of which ratings of mature individuals are to be made. Investigators of adult abilities who have used intelligence tests have been struck by an apparent decline of test scores after early maturity. On the basis of the largest collection of such data, in the U. S. Army investigations under Professor Yerkes' chairmanship, the investigators hesitated to posit an age-score decline. Rather it was suggested that the phenomenon might be accounted for by factors of differential selection and not by age as such. We are now able to present new data on this problem. Results obtained in the Stanford Later Maturity Study (directed by W. R. Miles), from giving the Otis S-A Higher Examination Form A as a 15-minute speed test (1) to about 2000 persons of whom more than 250 are 70 years of age or older, show correlations between age and score of approximately —.50 for adults between ages 20 and 95 at each of three educational levels.

Four questions are to be considered relative to these data: (*a*) The first is the question whether the trend observed in one population persists in others. It is answered in the affirmative by comparing four sizable populations.[1] The normal decline in scoring ability from the third to the ninth decade is in each of the four about 3.3 mental-age years of 25 IQ points on Otis' scale. The "average" individual who probably registers about half-way between our grade-school and high-school levels loses in scoring capacity from the third to the ninth decade at a rate of 6 to 7 mental-age months per decade. (*b*) The second question concerns the probability that similar score decrement will be found if the same individuals are tested at regular decade intervals from their early to late maturity. Results from a retest in 1932 of 190 persons first tested in 1930 shows approximately the same decline for a two-year period in each of four age segments, as is given by the age-score curve based on the large population of differing individuals (Table 1). The group of 190 persons is made

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[1]For a comparison of the first two of these, see Miles and Miles (2).

TABLE 1

Score decline in standard score units	Age periods 25-49 (41 cases)	50-59 (51 cases)	60-69 (55 cases)	70-89 (43 cases)
Expected	—.40	—.60	—.70	—1.20
Obtained	—.46	—.49	—.84	—1.42

up of 135 who returned voluntarily in the second experiment because of interest and 55 others who came because of our special and urgent request for the return of members of the 1930 population. Comparison of the scores of the 135 persons shows somewhat differing results from the above averages for the 190 (Table 2). Perhaps a practice effect not evident in the first comparison is registered here. In spite of this, the age influence seems to be progressively present.

TABLE 2

Score decline in standard score units	Age periods 25-59 (63 cases)	60-69 (44 cases)	70-89 (28 cases)
Expected	— .50	— .70	—1.20
Obtained	+1.75	+1.64	—1.03

(*c*) The third question concerns the factor of speed. Is the age decrement largely due to speed decrement? When Form B of the same Otis Self-Administering Examination series was given *without time limit* to 433 adults who had also taken the 15-minute Form A, age decrement was still found to occur, but the rate of decline from early to late maturity was not identical. In terms of standard scores the average decrement from decade to decade for A is .27 standard score units; for B, .24. In neither case does decline appear significantly before the fifth decade. From early maturity (20's, 30's, 40's) to later maturity (60's), the decline is .76 in standard score units for A, .46 for B; but from later maturity (60's) to old age (80's) it is .87 for A, .98 for B. We may say then that in early and middle maturity speed declines, on the average, rather faster than power, whereas in late maturity the decline of power is relatively more apparent.

(*d*) The fourth question concerns the extent to which the intelligence scores of adults register the intelligence of adults. The items contained in the tests in question make different psychological demands for their solution. It appears that where recognition rather than recall or the organization of new materials is required the decrement is not so great. This has been shown by a comparison of the item-by-item decrement in the Otis test. Further, in connection with quite another investigation, a study of sex differences conducted by Professor Lewis M. Terman and myself and

as yet unpublished, I have scored the item-information test, there given without time limit, for 30 people in each decade from the 20's to the 70's. Here recognition rather than recall is required and no problems involving new materials are presented for reorganization. With these conditions the scores rise definitely to a peak in the 40's and 50's to fall again to the early maturity level in the 70's.

Recognition of the speed-energy factor is, I hope, a first step toward the development of a new type of intelligence test standardized on the middle decades rather than on childhood and youth. Scores on this test must correlate for any decade group with the scores now available, but the serial means will show a developmental curve of a different sort from youth through early to late maturity. A test in which the speed factor is as far as possible eliminated and in which the experiential elements are registered for mature adults as adequately as they appear to be now for children and very young adults will, I expect, show not a curve of early decrement but a rise in score from decade to decade until somewhere in the late maturity period.

The practical value of our results measuring age-score decrement from early to late maturity is in the means which these norms furnish for rating older and younger persons on a single scale of comparison, that is, in terms of percentile rank or standard score within each decade. The curve for any given individual who takes the test under satisfactory conditions can be plotted and an estimate of his maximum rating in early maturity obtained. Of course, such a procedure is crude and has a wide margin of error, but, even so, it is considerably more accurate than any other mental-rating technique at present available for adults. A test score based on performance on two test-forms, preferably one with and one without time limit, has a fairly high validity. For studies of inheritance, comparisons of parents with children, and for individual clinical use, the decade percentiles offer a means of direct ability comparison between old and young. The present discussion has been based on intelligence score results on tests standardized for young people. Some suggestions as to the larger question of the status of intelligence in maturity have emerged from a consideration of these results.

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FIELDS OF INSTRUCTION AND RESEARCH IN PSYCHOLOGY AS REPRESENTED BY MEMBERS OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION

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What are the subjects which graduate students now preparing to teach psychology are most likely to be called upon to teach? This question is one which may be of interest to students in selecting their courses, and to Departments of Psychology in setting up minimum requirements. Of course it is realized that no altogether satisfactory answer can be given to the question. However, it does appear that a simple tabulation of what subjects are now being taught by members of the American Psychological Association would be of some value in this connection. This tabulation has been made for all full members of the American Psychological Association ($N=525$)[1] for the year 1932. A similar tabulation is being presented concerning the subjects of research. The original data were taken from the 1932 yearbook of the Association.

If these tabulations serve no other purpose than that of shaking some students out of the accumulating-of-credits attitude toward the selection and mastery of subjects not directly related to their research problem or major field of specialty, they will have accomplished a good deal.

The facts concerning the number of psychologists giving instruction in various subjects are given in Table 1; the number conducting research in various subjects is given in Table 2. The subjects in each table are given in alphabetical order. The terms used are those given in the yearbook. Since no classification of subjects in the field has been fully agreed upon, it was thought best to take down the facts just as they appeared in the yearbook and leave it to the reader to make any combination of subjects which appears logical to him. In a few cases where it was very obvious that two or more subjects should be considered at the same time cross references have been given in the tables.

The *Total* column in each table shows the total number of times that a given subject was mentioned. Since a given member might mention up to five or even six subjects, the *Total* column contains all subjects mentioned by all members. The number in the columns headed *1st, 2nd,* etc., refer to the number of times the subject was mentioned first, second, etc. Following each table there is a list of subjects which were mentioned by so few members as not to be listed in the table proper. In order to conserve space it was decided to list in the tables only those subjects which were mentioned at least three times.

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[1]Of these 525 there were 101 who reported subjects of research but no subjects of instruction; 19 who reported subjects of instruction but not those for research; 28 who reported neither.

TABLE 1
SUBJECTS IN WHICH INSTRUCTION IS GIVEN
(Number of members = 525)

		Total	Order of mention 1st	2nd	3rd	4th	5th	6th
1	Abnormal (see 24 and 25)	23	8	13	2			
2	Adolescent	3	1		2			
3	Advertising	8	3	5				
4	Aesthetics	8		6		1	1	
5	Animal (see 10)	3	2	1				
6	Applied (see 3, 18, and 21)	10	3	5	2			
7	Child (see 8 and 15)	19	9	7	3			
8	Child development (see 7 and 15)	3	2	1				
9	Clinical	45	28	14	3			
10	Comparative (see 5)	6	4	1	1			
11	Educational (see 12 and 28)	91	51	32	8			
12	Education (see 11 and 28)	16	3	10	3			
13	Experimental	48	17	23	5	3		
14	General	202	202					
15	Genetic (see 7 and 9)	12	4	4	3	1		
16	History	5		5				
17	Individual	3	1	1	1			
18	Industrial	20	3	10	7			
19	Mental hygiene	6	1	4			1	
20	Personality	6		1	4		1	
21	Personnel	4	1	2		1		
22	Philosophical	11	3	5	3			
23	Physiological	4	1	1	1			1
24	Psychiatry (see 1 and 25)	3	2	1				
25	Psychopathology (see 1 and 24)	27	4	11	9	2	1	
26	Social	24	3	9	9	2	1	
27	Statistics	26	3	12	6	4		1
28	Tests (see 11 and 12)	23	3	9	8	3		

The following subjects were mentioned either once or twice. The number following the subject indicates whether the subject was mentioned first, or second, or third, etc., in the list of subjects taught: Art (2, 2); athletics (4); behavior problems (2); child hygiene (2); conditioned reflex (1); correction of stammering (1); crime (1); development (1); differences (2); emotions (3); ethics (2); family (2); feelings (4); individual differences (2); learning (1, 2); legal (4); manufacturing (1); motor (3); music (1, 2); neurology (4); otology (2); ophthalmology (3); optics (2); parent education (3); primitive people (3); psychophysics (2); racial (1, 2); recreation (1); religious (1, 1); research (1); research methods (1); selling (3, 3); social work (1); sociology (2); speech (3, 4); systematic (2, 3); systems (4); theory, quantitative (1); vision (2); vocational guidance (2).

TABLE 2

SUBJECTS IN WHICH RESEARCH IS CONDUCTED
(Number of members = 525)

		Total	Order of mention 1st	2nd	3rd	4th	5th	6th
1	Abnormal (see 28 and 30)	41	36	5				
2	Advertising	10	8	1	1			
3	Aesthetics	18	4	9	1	4		
4	Animal (see 8)	27	20	5	2			
5	Applied (see 2, 17, and 25)	17	11	4	2			
6	Child	20	13	3	3	1		
7	Clinical	88	60	27	1			
8	Comparative (see 4)	20	11	7	1	1		
9	Criminal	3		3				
10	Educational (see 11 and 37)	123	74	35	11	1	1	1
11	Education (see 10 and 37)	16	1	7	5	3		
12	Experimental	135	66	58	10	1		
13	General	66	59	5	1	1		
14	Genetic	31	13	10	6	1	1	
15	History	8	1	4	1	2		
16	Individual	7	3	4				
17	Industrial	28	7	14	7			
18	Language	5	2	2			1	
19	Learning	11	3	4	3	1		
20	Mental hygiene	6	1	4		1		
21	Music	3	1	1	1			
22	Neurology	7	2	1	2	2		
23	Optics	3		1	2			
24	Personality	11		1	5	3	2	
25	Personnel	9		4	4	1		
26	Philosophical	11	3	5	2	1		
27	Physiological	28	2	17	8		1	
28	Psychiatry (see 1 and 30)	16	2	4	4	4	1	1
29	Psychoanalysis	3	1	1	1			
30	Psychopathology (see 1 and 28)	28	3	13	7	4	1	
31	Racial	3		1	2			
32	Religious	10	5	2	3			
33	Selling	3		1	2			
34	Social	51	4	23	18	5		1
35	Speech	3	1		1		1	
36	Statistics	20		5	11	3	1	
37	Tests (see 10 and 11)	122	6	25	47	28	14	2
38	Theoretical	35	1	16	15	2	1	
39	Vocational	36	7	13	11	5		

The following subjects were mentioned either once or twice. The number following the subject indicates whether the subject was mentioned first, second, third, etc. Art (1); adolescent (1, 2); behavior problems

(2); blind (3); body chemistry (2); business (1, 1); chain-store merchandising (1); child development (1, 1); child health (1); color (3); curriculum (3); delinquency (3); differential (3); drugs (2); emotions (4); ethics (4); feelings (3); guidance (3, 5); individual differences (2); inhibition (1); laboratory photography (2); legal (2); logic (2); mental development (4); mental life (2); memory (3); merchandising (1); motivation (4); neurological (2); neurological physiology (4); opium addiction (3); ophthalmology (4, 4); personality differences (1); phonetic (2, 2); political (3); prison (3); psycho-biological (4); psycho-clinical (1); psychology of school subjects (3); reading (3); scientific method (4); sex in marriage (1); social work (1); theory, quantitative (1); thought (2); vision (2, 2).

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STUDIES OF WARMING-UP IN A PERCEPTUAL-MOTOR TASK: SERIES II

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This report is the second of a series of studies made by the writer of the phenomenon of WU (warming-up) in the case of a task of perceptual-motor ability. The first series of studies was reported in 1931 (2). As the apparatus and general physical set-up, as well as the general procedure, have already been described in the earlier publication, only a very brief statement will be made here. The task consisted in replacing the blocks of a Goddard formboard under carefully standardized external conditions and instructions to the subject. In all but two cases herein reported, the blocks, at the beginning of each trial, were placed in the recesses of a second Goddard board which was placed just forward of the formboard into which the blocks were to be placed and in a corresponding spatial position. The recesses of this board, in which the blocks were placed at the beginning of each trial, were filled with cardboard so that the blocks projected above the board three-fourths of an inch. All experiments were performed in a dark-room under standard light conditions as described in the earlier report. The temperature of the room varied from 72 to 76 degrees, usually being about 74 degrees. All subjects used in this investigation were college men.

The present investigation was carried out in the hope of gaining some information on the following problems:

1. The effect of length of rest interval between trials on WU
2. The effect of length of task on WU
3. The relation between degree of skill and WU

4. The relation between complexity of task and WU
5. The effect of a preceding mental or muscular exercise on WU

The Robinson-Heron formula (1) was used in testing for the presence or absence of WU. Only percentage values are given in this report as the writer is convinced that such values are, at present, the most trustworthy. Under every given condition we have found what percentage of the total records obtained show a positive WU effect.

Experiment I:
Effect of Length of Rest Interval between Trials

Two groups of college men served as subjects in this experiment, 20 men to a group. After a practice session on a Friday, every subject worked the formboard 20 times (trials) on each of five consecutive days, beginning on the following Monday. Since, using the Robinson-Heron formula, the last day of work can be used only as a check on the preceding day's work, we obtained four WU records for every subject. Group A rested 30 seconds between each trial, while Group B rested 60 seconds. During the rest the subject was instructed to relax as much as possible. Little or no conversation ensued during the rest other than a few occasional remarks about the preceding trial or the work in general. The subjects knew that they were working on the general problem of WU but had no detailed knowledge of the specific problem upon which we were working.

When the subject stepped up at the beginning of each trial the blocks were all in their proper places in the container-board and in a corresponding spatial position as regards the receiving board. Every subject was told that we expected him to become as expert as possible during the time that he had in which to work the board. At the beginning of each day's work the following instruction was given to the subject: "Remember that today you are going to do your best from the very beginning. You are going to step up and make the best record that you can at every trial."

When the subject appeared on Friday for the introductory work we explained carefully just how the blocks were to be replaced and worked the board once while he observed. The ten blocks were numbered by pairs, as 1,1, 2,2, 3,3, etc.[1] The two number-1 blocks were to be replaced first, the subject using both hands at the time. The two number-2 blocks were to be replaced next, and so on. The numbers were printed in large black type on every block so that they were easily seen.

Every subject then proceeded to work the board two times without being timed. He then worked the board ten times and was carefully timed on each trial. This gave the subject considerable practice in the work and enabled us to gain some measure of the subject's initial formboard ability. We attempted to have all of our groups about equal in initial formboard

[1] 1,1, Diamond, Star; 2,2, Circle, Oval; 3,3, Oblong, Square; 4,4, Cross, Hexagon; and 5,5, Triangle, Semicircle.

ability and succeeded quite well in this regard. Thus, the average times for the first ten trials for Groups A and B were, respectively, 5.72 and 5.99 seconds.

The results of this experiment are shown in Table 1. The data indicate that WU occurs more often when a 30-second rest interval is used between trials than when a 60-second interval is interposed.

TABLE 1

Group A, 30-second rest intervals, showed WU in 54.4% of records
Group B, 60-second rest intervals, showed WU in 35.0% of records

Experiment II: Effect of Length of Task

Using the same general physical arrangements of the task and the same general procedure as that in Experiment I, a comparison was made between the amount of WU in the case of Group A (considered above), which worked the board 20 times at each meeting, and Group C, a new group of 15 men, that worked the board but 10 times at each meeting. My assistant, Mr. E. Polzin, worked with Group C, as was also the case of Groups D and E, which will be considered later. The results are shown in Table 2.

TABLE 2

Group A, 20 trials, showed WU in 54.4% of their records
Group B, 10 trials, showed WU in 48.0% of their records

These data indicate that there is likely to be more cases of WU when the task is longer. However, due to the fact that Group C was somewhat smaller and due to the fact that a different experimenter worked with each group, the above suggestion is to be regarded as only tentative.

Experiment III: Effect of Degree of Skill

Does one show more or less WU as one becomes more practiced or skilled in a given task? In our previous study we tentatively concluded as follows: "The degree of skill attained seems not to be of any particular significance in our work." On the other hand, the present study seems to indicate some rather definite relation between degree of skill and the percentage of records showing WU. Table 3 shows the percentages of occurrences of WU in the case of Groups A and B, using 20 trials per meeting. The upper two rows of figures are based upon a comparison of the first two days of regular work (Monday and Tuesday) with the last two days of usable records (Wednesday and Thursday). The lower two rows of figures are based upon a comparison of the first and last (Monday and Thursday) meetings. As has been stated, the Friday records can serve only as a check upon the Thursday records when the Robinson-Heron formula is used.

TABLE 3

Days compared	Group A	Group B
% of records of Mon. and Tues. showing WU	47.5	27.5
% of records of Wed. and Thurs. showing WU	61.5	42.5
% of records of Mon. showing WU	30.0	25.0
% of records of Thurs. showing WU	69.0	40.0

The data of Table 3 indicate rather definitely that as the subject becomes more skilled in the task the larger is the percentage of WU records.

The data for Groups C, D, and E, which are presented in Table 4, are not so clear cut. Before inspecting the records of these groups it is necessary to describe the conditions under which these groups worked. Group C worked under the same conditions as did Group A with the exception that 10 trials instead of 20 were used. Group D followed the same procedure as Group C except that the container-board was placed in a reversed spatial position in relation to the board which was to receive the blocks. This set-up, according to the amount of time needed per trial and according to the subjects' introspections, was more complex and difficult than that used in the case of Group C.

The physical set-up for Group E was the most complex and difficult of all. Instead of having the blocks always in a fixed position at the beginning of every trial, the blocks were mixed up in a random arrangement on a prescribed flat surface formed by turning over and using the bottom-side of the container-board. The task of Group C, then, was easiest and that of Group E was the most difficult, while that of Group D was intermediate.

TABLE 4

Days compared	Group C	Group D	Group E
% of records of Mon. and Tues. showing WU	56.7	26.7	43.3
% of records of Wed. and Thurs. showing WU	40.0	50.0	50.0
% of records of Mon. showing WU	46.7	40.0	53.3
% of records of Thurs. showing WU	46.7	46.7	53.3

The data of Table 4 are anything but enlightening. Groups D and E seem to indicate more WU with increase in skill if we compare the first two and last two days of regular work. However, if we compare the first and last days of work the averages for Group E are the same, namely, 53.3 seconds.

The data of Table 6 offer some further suggestions. The writer and Mr. Polzin each served as subject, working the board 10 times at each daily meeting with a rest of 30 seconds between trials. These two subjects were highly trained in the work. An inspection of Table 6 shows that the per-

centage of cases of WU is relatively large in the case of these two highly trained and well-motivated subjects.

Experiment IV: Effect of Degree of Complexity of Task

We have already referred to the data of Groups C, D, and E in connection with Experiment III. These three groups were used primarily in order to find the relation, if any, between the degree of difficulty (complexity) of the task and the amount of WU. As has been stated, Group C had the easiest set-up, Group D had a set-up of intermediate difficulty, and Group E had the most difficult task. Table 5 presents the findings.

TABLE 5
Percentage of Records Showing WU

Group C	Group D	Group E
48	43.3	46.6

The above data do not justify any definite conclusion. The simplest set-up, used by Group C, shows the greatest percentage of cases of WU, but the difference between the simplest and most complex tasks is very small.

Experiment V: Effect of Preceding Mental and Muscular Exercise

Unfortunately, we have but two subjects upon which to base any conclusions in this experiment. Hence our data can be regarded only as suggestive at best. These two subjects, already mentioned, were quite expert in the task and certainly tried on all occasions to do their best. The data for each subject are based upon 45 successive days of work, 15 days for each of the three conditions investigated. The three conditions were rotated by weeks.

In Condition X the subject stepped up to his task with no preceding mental or muscular exercise. Rather, he had just been relaxed to a normal degree. In Condition Y the subject indulged in a moderate arm and finger exercise for 30 seconds just before the task. In Condition Z the subject spent 30 seconds studying and mentally rehearsing the work as he looked at the set-up. Table 6 presents the results.

TABLE 6

Subject	Condition X	Condition Y	Condition Z
S	46.7%	53.3%	53.3%
P	62.5%	66.6%	71.4%

Condition X, a condition in which no systematic mental or physical exercise preceded the work, shows the smallest percentage of cases of

WU. Evidently, something occurred in the preceding activities of Conditions Y and Z which operated to increase the number of cases of WU.

Summary

Our data, obtained from college men engaged in a perceptual-motor task, suggest the following conclusions:

1. A larger percentage of cases of WU was found when a rest interval of 30 seconds was used than when a rest interval of 60 seconds was used.
2. The longer task of 20 trials resulted in more cases of WU than the shorter task of 10 trials.
3. In the case of the longer task the more practiced or skilled subject showed more cases of WU. In the case of the shorter task the results were inconclusive.
4. We were unable to demonstrate any marked effect of degree of complexity or difficulty of task upon WU.
5. In the case of two highly trained subjects both a short preceding arm-hand-finger exercise and a brief study and mental rehearsal of the task resulted in more WU than a preceding condition of normal mental state of relaxation and no exercise.

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THE CONDITIONED REFLEX AS THE EXPLANATION OF HABIT FORMATION: II. THE OPERATION OF A HIGHER-ORDER REACTION AND A LOWER-ORDER REACTION IN CLOSE SUCCESSION

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The possibility of explaining habit formation as conditioning has been suggested by a number of psychologists. This experiment is one of a series attempting to shed light on the adequacy of such types of explanation. Two previous papers have appeared, one (1) indicating the nature of the problem and the other (2) examining the relative rôles of the temporal factor and of the dominance factor in the establishment of the conditioned reflex. In this latter study, which was confined to two lower-order reactions, the temporal factor seemed the more potent. This makes it somewhat

difficult to explain habit formation on the basis of the mere coincidence of an indifferent stimulus and a stimulus initiating a dominant reflex.

In the above study the criterion of dominance was that of intensity of shock. The more intense shock was regarded as initiating the more dominant reflex. Admittedly this is not a perfect criterion inasmuch as, in the Pavlovian scheme, the dominant reflex differs from the indifferent reflex in some qualitative fashion. A difference in intensity was used because it made possible a definite gradation of differences which would be impossible using two qualitatively different reflexes. That is to say, in the case of the shocks we could set the intensity the same or different and thus secure steps of dominance difference varying from *0* to *n*, whereas if we used two qualitatively different reflexes we could never be sure that the difference in dominance was zero.

In the present experiment I used two qualitatively different sensori-motor arcs. For one I continued to use the reflex withdrawal from shock and for the other a voluntary response to a signal. In the case of the latter reaction the subject held a small hammer in his right hand and was told to look for the flash of a light and, as soon as the light flashed, to try to hit a metal rectangle about 1″ by 1½″ placed on the distal end of an arm rotating in a horizontal plane. The arm bearing the target made approximately 2 rotations every 3 seconds. The target and the hammer could be seen only by their reflection in a mirror.

It will be seen that the connection between a shock in the left hand and the withdrawal of the finger was of a radically different order from that between the flash of light and the response of hitting at a target. To be sure, I know of no definition which would enable us to determine exactly which of these tendencies is the more dominant. In view of the general *connotations* of the word, however, I would have assumed that the withdrawal reflex is the more impelling. This is, however, a mere assumption. It would be interesting for the reader to decide for himself at this point which he would consider the more dominant.

Since the apparatus was taken over almost unchanged from the previous experiment, the complete technical description will not be repeated here. The electrode for the right hand was removed and a small flashlight bulb substituted so that a flash of light would replace the application of shock to the right hand. A mirror and revolving target were installed, and near the latter was placed a small mercury cup in which the metal head of the hammer rested while the subject waited for the light to flash. The removal of the hammer from this pool broke a partial short circuit across the terminals of the recording needle thus causing an increase in the amplitude of the latter.

The procedure differed from that of the previous study only in that no variation of intensity was attempted. On account of this simplification

only three groups were required. With one group (L—S) the paired stimuli consisted of a flash of light followed after an interval of ¼ second by a shock in the left hand. With a second group (S—L) the shock was given first and the flash of light followed after ¼ second. The members of the third group (simultaneous) received both stimuli simultaneously.

The procedure with each subject was as follows: The experimenter demonstrated the manner of holding the stylus and of hitting at the target, at the same time calling attention to the printed directions which indicated that the subject was to hit at the target as soon as he could after the light flashed. The experimenter then showed how the left hand should be placed on the electrode and again called attention to the printed directions which read, "Try to let the left hand take care of itself except for replacing it on the electrode should it jerk away after a shock. Do not try to prevent the hand from jerking away as that would make the shock much worse." The subject then seated himself and was allowed to practice placing the hammer in the pool and hitting at the target while the latter remained stationary. A screen was then placed around him, the target was put in rotation and the light flashed in succession several times. Following this a series of four or five shocks was administered and the inductorium adjusted so that a withdrawal response regularly followed the shock. (In the case of most subjects the intensity of the shock had to be frequently increased during the course of the experiment.)

As soon as enough single stimuli were given to insure that a definite discrete response followed each type of stimulus respectively, a series of paired stimuli was given. At certain intervals a trial, consisting of only one stimulus, would be given to determine if conditioning had been established. The arrangement of these trials can be determined from the following schema: 11L, 16S, 20S, 26L, 29S, 34L, 36L, 39S, 42L, 50L, 60S, 66S, 74L, 79S, 86S. This indicates that after 10 stimulations the light was flashed alone, and after 15 stimulations the shock was given alone and so forth. Half of the subjects in each group followed the above order. The other half followed an order in which the succession of S and L trials was directly reversed. That is, the shock was given alone after 10 stimulations and the light was given alone after 15 stimulations. After 92 stimulations the light (in the case of half of the subjects) was successively presented alone until any conditioned reaction to it (withdrawal of the left hand) was extinguished. A series of two paired stimulations followed and then the shock was presented alone until any conditioned reaction to it (hitting at the target) was extinguished. In the case of the other half of the subjects the conditioned reaction to the shock was extinguished first. Altogether this usually involved about 100 stimulations for each subject, covering a period of slightly less than 30 minutes.

TABLE 1

SHOWING THE APPEARANCE (C) OR NON-APPEARANCE (X) OF CONDITIONING AT EACH TRIAL (UNREINFORCED STIMULUS) OF BOTH TYPES FOR ALL INDIVIDUALS; THE DIFFICULTY OF EXTINGUISHING CONDITIONED REFLEXES OF EACH TYPE, AND THE PERCENTAGE OF "SUCCESSFUL" (C) TRIALS OF EACH TYPE

	No. of subject	11 S	11 L	16 S	16 L	20 S	20 L	26 S	26 L	29 S	29 L	34 S	34 L	36 S	36 L	39 S	39 L	42 S	42 L
S—L																			
	11		X C X C	C C — C		C C C C			X X X X	C C C			C C X C		X C C X	C C C C			C C C X
	12																		
	13																		
	15																		
	16	C			X		X	—	X	C	X	C			X	X		X	
	17	X			X		C	C —			C	C		C			X	X	
	20	X			X		C				C	X					X	X	
	21	—			X		X	C	C	—	C	—		C			X	X	
% success																			
	S	33		100		100		100		100		67		100		80		0	
	L		50		0		50		0		75		75		40		0		75
L—S																			
	2		C X C	C C —		C C —			X X X	C C X			X X X		X X C	C C C			X X X
	4																		
	5																		
	6	C			C		C	C			C	C		C			C	C	
	7	C			C		C	C			C	—		C			C	C	
	8	C			C		C	C			X	C		C			X	C	
	9	C			X		X	C			X	—		X			C	C	
	10	C			X		X	C			C	—		—			X	C	
% success																			
	S	100		100		100		100		67		100		75		100		100	
	L		67		60		60		0		60		0		0		60		0
Simultaneous																			
	27		X X X C	C C C		C C C			X X X	X C C			X X X		X X C	X C C			X X X
	28																		
	26	—			X		C	C			X	—		—			X	X	
	29	X			C		X	C			X	X		—			X	X	
	30	—			X		X	C			X	C		—			X	X	
% success																			
	S	0		100		100		100		50		50		—		50		0	
	L		0		33		33		0		0		0		0		0		0

TABLE 1 (*continued*)

50 S	50 L	60 S	60 L	66 S	66 L	74 S	74 L	79 S	79 L	86 S	86 L	No. of single stimuli required to extinguish each type S	No. of single stimuli required to extinguish each type L	Type extinguished first	% of success trials of each type for each individual S	% of success trials of each type for each individual L
	X	C		C			X	C		C		7	0	L	100	29
	C	C		C			X	C		C		6	1	L	100	71
	C	—		—			C	C		C		2	0	L	100	57
			X				X	C		C		9	0	L	100	43
—			X				X		X		X	1	0	S	50	0
X			X		X	C			X		X	1	0	S	57	25
C			X		X	X			X		X	1	0	S	20	25
X			X		X	X			X		X	1	1	S	40	12
33		100		100		33		100		100		3.5			76	
	75		0		0		20		0		0		0.2			31
	C	C		C			X	X		C		1	0	L	87	29
	X	C		C			X	C		C		4	0	L	100	0
	X	—		C			X	C		C		12	0	L	80	14
C			C		C	—			—		—				100	100
X			C		C	C			C		C	1	2	S	83	100
—			C		C	C			C		X				100	62
X			X		X	C			X		X	0	0	S	57	12
C			X		X	C			X		X	1	0	S	100	12
50		100		100		75		67		100		3.0			89	
	33		60		60		0		50		25		0.3			36
	—	C		X			X	X		X		0	3	L	37	0
	X	C		C			X	C		—		1	0	L	100	0
			C		C	X			X		X	0	0	S	33	37
—			X		X	C			X		X	0	0	S	50	12
C			X		X	C			X		X	1	0	S	60	0
50		100		50		67		50		0		0.4			55	
	0		33		33		0		0		0		0.6			9

The results are given in some detail in Table 1. At the extreme left of the table is given the group to which the subject belongs and his identification number. The numbers *11, 16,* etc., indicate that the trial occurred at the 11th, the 16th, etc., stimulation. *S* indicates that at that trial the shock was given alone, *L* that the light was given alone. The letter *C* indicates that conditioning was observed at that trial (i.e., that *both* responses followed the single stimulus) while *X* indicates that no conditioning was observed (i.e., the stimulus was followed by its proper response only). A dash means that no response followed the stimulus. Following the column headed 86 is given the number of successive single (unreinforced) presentations of the shock (under column *S*) and of the light (under column *L*) required to extinguish any conditioned response attached to them. The next two columns give for each subject the percentage of *S* trials and of *L* trials in which conditioning was observed.

Thus for Subject 15 the table would read: After 10 paired stimuli (i.e., at the 11th stimulus) in which the shock to the left hand occurred ¼ of a second before the flash of the light, the light was presented alone and was followed by an attempt to hit the target and by a withdrawal of the left hand. After 15 stimulations the shock was presented alone and was also followed by both reactions; on the 20th stimulation the shock alone, followed by both reactions; on the 26th the light alone, followed only by the attempt to hit the target. Skipping to the 60th, we find that the shock was presented alone and followed by neither reaction (presumably not felt). Passing over the trials, we notice that 9 applications of the shock alone were presented before the subject refrained from attempting to hit the target on the occurrence of the shock, that 0 presentations of the light alone were required to extinguish the conditioned withdrawal to it, and that the extinction of the conditioned reaction to the light preceded that of the conditioned reaction to the shock. The last 2 columns indicate that conditioning occurred in 100% of the trials in which the shock was presented alone and in 43% of the trials in which the light was presented alone.

The results for each experimental group are summarized by indicating the percentage of successful trials of both types at each serial position of the trials and the unweighted average of these, and the average number of trials of each type required to extinguish the conditioned reflex.

A chance for error occurs in the use of the unweighted averages mentioned above. To correct for this I found the percentage of all trials in which the shock was given alone (irrespective of subjects or of the serial position of the trial) and the same for all the trials in which the light was presented alone. The figures for the respective groups are as follows:

Group	% of successful shock trials	% of successful light trials	Difference
Shock followed by light	76	32	44
Light followed by shock	88	41	47
Light and shock given simultaneously	59	11	48

These figures are substantially the same as those obtained for the unweighted averages.

By whatever method we analyze the results, several rather definite facts appear.

1. Some connection forming or learning or modification has taken place during the experiment. At the beginning of the experiment the subject tended neither to hit at the target upon the occurrence of the shock, nor to withdraw his hand upon the presentation of the light. At the end of the experiment both of these "cross" tendencies were present in some degree.

2. The learning or crossing was predominantly of one type. Although the subjects came both to hit when the shock came, and to withdraw the hand when the light flashed, the former tendency was much more definite than the latter. The shock-hit connection functioned in about 77% of the opportunities given, while the "light-withdraw" bond was noticed in only 30% of the trials.

3. The tendency of the learning to be predominantly of one type was found in all three groups. No matter whether the light was presented first, or the shock first, or both together, there were about 45% more hitting reactions to the shock than withdrawal reactions to the light.

4. While the difference between the two types of learning remained relatively consistent from group to group, the absolute amount of learning (of either type) varied. Both types were apparently handicapped by the simultaneous presentation, although when an interval did appear it seemed a matter of indifference which stimulus came first.

It is difficult to see how the above facts could be explained by the conditioned-reflex schema. This difficulty is the more interesting because the set-up of the experiment differs very little from that of the typical example of conditioning outlined in the elementary textbooks. The results, however, are directly contradictory to the results of that typical experiment. In the typical experiment the light is flashed and a shock follows. After a few repetitions of this procedure the light comes to elicit the withdrawal reflex. In the present experiment the procedure was modified only in telling the subject to respond to the light in a certain arbitrary way. Now, however, the direction of the "conditioning" is reversed and the shock comes to elicit the response arbitrarily attached to the light.

In order to explain the above fact in terms of the conditioned-reflex schema we would have to assume that the light-hit reaction was the *unconditioned* or more *dominant* reflex and that the shock was an inadequate or conditioned or neutral stimulus. While I know of no definition of an unconditioned reflex which would make it impossible, such assumption would certainly not accord with our ordinary ideas of the meaning to be attached to the terms "unconditioned" or "dominant." Such an unconditioned reflex would not be innate or primary in any sense of the terms. Moreover, it seems ridiculous to suggest that the tendency to hit at a target whenever a light flashes is in any way less dependent upon "conditions" than the tendency to withdraw the hand from a shock.

The slight rôle played by the temporal factor is also significant. In conditioning the salivary response the temporal factor has been considered highly important. In the association of the two lower-order reactions described in the preceding study of this series, it was found to be the most important factor. In this experiment, however, the temporal factor seems without significance as far as the direction of the conditioning is concerned. The varying importance of the temporal factor would seem to suggest that the type of learning going on in the establishment of a conditioned salivary reaction is not the same as that going on in training a subject to hit at a target upon the occurrence of a shock.

In general, the results indicate that the conditioned-reflex schema is not a very dependable means of establishing higher-order habits. Let us consider, for instance, the task of training a child not to reach for a vase. According to the proponents of the conditioned-reflex theory, we could accomplish this by presenting the vase to the child and immediately thereafter giving him a shock. After several applications of the treatment the child should come to shrink away from the vase whenever it is presented to him. If, however, my results are typical of what happens, we would expect rather that he would be much more likely to come to respond to the shock by reaching for the vase than to come to respond to the sight of the vase by withdrawing. There is no suggestion, of course, that the results of this experiment should be applied to such a case. The suggestion is limited to the necessity for more careful examination of the concept of the conditioned reflex before applying it unreservedly to the explanation of other forms of habit formation.

Summary

Twenty-one subjects were instructed to respond to a flash of light by hitting with the right hand at a revolving target. The light was then presented about 100 times in conjunction with a shock to the left hand. In the case of 8 subjects the light appeared ¼ second before the shock, in the case of 8 others ¼ second after shock, while in the case of 5 subjects the light and

shock were presented simultaneously. In all groups there was distinctly more evidence of a tendency to react to the shock by striking at the target than to react to the light by withdrawing the left hand. It is held that these results cannot be explained by the Pavlovian schema without considering the reaction of hitting at the target when the light flashes as the unconditioned or more dominant reflex. Stretching the concepts of unconditioned or dominant to this extent would, it is argued, rob them of all significance.

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TACTUAL SPACE PERCEPTION IN TRANSLOCATED TISSUE

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It is well known that amputation, e.g., of a limb, is often followed by phenomena of illusory tactual localization, which may persist for a long period of time. Stimulation of the stump of an amputated arm gives rise to a perception of pressure or pain in a non-existent hand. In other words, the impression is localized in the place where the stimulated nerve fibers would normally end. Analogous phenomena have been noted as a consequence of surgical operations in which a piece of skin was transplanted from one part of the body to another, without severing all the original nerve connections of the transplanted area. Where an artificial nose is constructed out of a piece of skin from the forehead, the patient may perceive a touch applied to this transplanted tissue as located on the forehead. As the gradual regeneration of new nervous connections proceeds, this illusory localization comes to be replaced by the normal mode.[1]

What will happen in a case where the position of a piece of peripheral tissue is changed, but where a large part of the original nerve supply is not interrupted, so that there is no far-reaching readjustment due to nerve regeneration? Will normal localization be developed through "experience," or will the illusory form of localization stubbornly persist? Since the literature seems to furnish but scant information on this point, it may be worth while to present a brief report dealing with a relevant case.

[1]Cf. V. Henri (2, pp. 151 ff.).

The subject, B, is a university undergraduate, aged 23. Ten years ago (1923) he experienced an accident in which the end of the middle finger of his left hand was nearly, but not quite, severed at the first joint. An operation was performed in which this terminal phalanx was removed, leaving a large part of the skin belonging to its volar side still in continuity with the rest of the finger. This volar skin was brought up to cover the joint, and stitched to the uninjured skin of the dorsal side of the second phalanx. The line of suture is about 6 mm. proximal to the present end of the finger.

The translocated skin is distinguishable from the adjoining normal skin by its soft and pinkish appearance, although the boundary line is not very sharp. The displaced tissue is abnormally sensitive to pressure, and a very moderate pressure gives rise to an experience of pain. It is extremely sensitive to warmth, as the following example will illustrate. The point of a thermo-aesthesiometer was heated to such a temperature as would just barely give rise to a perceptible warmth when applied to the volar side of the wrist. No warmth could be felt at all when the same stimulus was applied to the normal skin on the dorsal side of the finger in question, at a place just proximal to the boundary line. If, however, the stimulating point was brought near the end of this finger, warmth was felt before the point had even touched the skin. As soon as the point had come within 3 or 4 mm. of this part of the finger, B reported that he felt "waves of heat" impinging on it (his eyes being closed). This hypersensitivity was confined to warmth, and did not appear in the case of cold stimuli. As a matter of fact, sensitivity to cold is slightly lower in the translocated skin than in the adjoining areas.

Stimuli applied to the abnormal region are invariably localized in an illusory fashion. If any part of this region is touched, the resulting impression always appears to the subject as located on the volar side of the non-existent first phalanx. According to his very positive statement, the tactual effect is exactly the same no matter whether he closes his eyes or whether he opens them and looks directly at the stimulated finger. The persistence of the illusion, despite years of constant conflict between tactual and visual data, seems worthy of note.

As a rule, stimulation of the translocated skin is not localized on the *surface* of the first phalanx; B declares that the sensation seems to be "inside" this missing part, and towards its volar side.

If pressure is successively applied at points on the dorsal side of the abnormal region which are farther and farther removed from the line of suture, the felt pressure is localized on the volar side, at first quite near the end of the missing phalanx, then progressively nearer to the volar side of the first joint. On the volar side of the actual finger one finds the illusory localization shading into the normal localization by continuous transitions.

Particulary interesting is the phenomenon reported when one applies a touch to the line of suture. In this case the touch is felt as *double,* in other words is localized in the normal and the illusory manner at the same time. This is no doubt because such a pressure necessarily excites afferent fibers of two kinds—those belonging to the translocated tissue and those belonging to the neighboring normal tissue. (This phenomenon occurs even when the skin is touched very lightly with a horsehair. Even under these conditions there is probably considerable peripheral spread of stimulation.)

Ordinarily, according to B, the injured member feels like a perfectly normal and complete finger. When, however, he was asked to plunge the finger into a vessel of cool water, he reported a very curious effect. According to his impression, the dorsal side of the terminal phalanx was now very definitely missing; the finger felt as if a large slice had been taken out of it. Evidently, the stimulation of the whole surface by cold furnished a vivid background in which the absence of the part which lacked peripheral representation could be perceived as a definite gap in the sensory field.[2]

When a fine point was moved along the dorsal side of the finger in the direction of the end, there was an apparent discontinuity in the movement at the instant of crossing the suture line. At this moment the point seemed suddenly to cease its movement along the dorsal side; it apparently "popped up" on the volar side and began to move in the opposite direction.

If two fine points were pressed simultaneously against the skin on either side of the line of suture, B felt one point as situated on the dorsal side, and the other as situated on the volar side, with the thickness of the finger between them. This tactual impression was not modified by visual awareness of the actual conditions of stimulation.

Form perception within the abnormal area was very poor. Small objects, of whatever form, which were pressed against this part of the finger were usually felt simply as diffuse spots of no definite shape. A line (the edge of a card) felt like a "blunt point" when it was applied near the middle of the abnormal region. Towards the edges of this region, on the contrary, it was perceived as a definite straight line, but the line as a whole was erroneously localized.

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[2]One might expect that the gap would perhaps be "filled in" by a central induction of the warmth sensation (temperature contrast)—in analogy to the induction of contrast colors in scotomatous areas of the visual field. No such contrast effect appeared, either with cold or warm water. In any case, the reality of so-called temperature contrast has been called in question by recent research [cf. Hahn and Lueg (1)].

ABSOLUTE TEMPO

PAUL R. FARNSWORTH, HAROLD A. BLOCK, AND WARD C. WATERMAN

The problems of absolute or positive pitch have been studied by a considerable number of researchers. The same cannot be said of tempo, the rate or pace at which music is played.[1] How, for instance, does the average university student regard waltz and fox-trot tunes in terms of metronome rates? Is some one tempo consistently associated with each of these dance types? It was to answer these questions in some measure that the present studies were planned.

Each subject was blindfolded and placed in front of a Duo-Art reproducing piano. The tempo lever was placed in his hand, and either a waltz or a fox-trot was played to him. He was allowed to listen to as much of the piece as he wished. His directions were as follows:

> "This test seeks to measure your ability to judge the proper tempo for waltz time (3-4) [fox-trot time (4-4)]. You are to listen to a waltz [fox-trot] tune played on the piano. The lever at your right hand regulates the tempo of the music. Place this lever at the position you consider to give the proper tempo to the music. Make four (4) separate tests, stopping the music after each test, and starting it again when signaled to do so."

The directions were amplified verbally when necessary. The subject was also placed at a telegraph key and given these directions:

> "This test seeks to measure your ability to tap the proper rate (tempo) for modern dance music (4-4) [waltz time music (3-4)] much as if you were the drummer in a dance orchestra with instructions to give four (4) [three (3)] steady beats per measure—1-2-3-4-1-2-3-4, etc. [1-2-3-1-2-3, etc.]. You are to keep your tappings as consistent as possible. That is, do not start rapidly at first, and slow down later. If you start at a certain rate (tempo) remain as nearly to that standard as you possibly can. There will be four (4) tests in all, each test lasting for one-half (1/2) minute. Before the start of each test you will have two (2) minutes in which to practice. After this practice period do not touch the telegraph key until you are signaled to do so. After each test there will be a one- (1-) minute rest period. The following signals will be employed: THREE BUZZES——READY (practice period is over; prepare for start of actual test); TWO BUZZES ——GO (commence tapping); ONE BUZZ——STOP (cease tapping). Remember to be consistent."

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[1]Alfred Guttmann (2) has measured the tempi at which orchestras under several different directors have played a given piece. Stetson and Tuthill (4) have measured rhythmic unit-groups while varying the tempi.

The first subject listened to a fox-trot played by the Duo-Art,[2] and then was asked to tap the same rate on the telegraph key.[3] The second subject tapped fox-trot tempo first, and then heard this tempo rendered by the Duo-Art. The third subject listened to a waltz played by the Duo-Art, and then tapped this tempo on the key. The fourth subject tapped waltz tempo first, and then listened to it as played by the piano. Later subjects followed in order the cycles of the first four. It should be noted that each subject listened to and reproduced either 3-4 or 4-4 tempo—never both.

Thirty-five subjects chosen at random from classes in elementary psychology completed the first set of experiments (Table 1). The waltz roll employed was Duo-Art Number 0846, entitled "Why Dance?" The fox-trot roll was Number 0847, "Time On My Hands." A second set of experiments (Table 2) with 40 subjects was run with different pieces to check the first and to obtain retest reliabilities. The rolls used were "Rockabye Moon," Number 0902, a waltz, and "Isn't it Heavenly?," Number 0930, a fox-trot. In both experimental series the taps were recorded on a modified Weiss-Renshaw polygraph which was one room removed from the subject. In the first series the time line was in part obtained from a pendulum clock. As the recording was not uniformly good (several records were rendered valueless), the time line was subsequently obtained from a well-calibrated synchronous motor. The retests of Series 2 were obtained two days after the first testings.

The data reported in the tables below are from the subjects' first scores. (Each subject gave 4 tapping responses and 4 settings of the lever.) The other three were employed in the estimation of reliability values. The first and fourth were averaged, and correlated against the average of the second and third. The resultant figures were raised by the Spearman-Brown formula. The values were: 4-4 tapping, .96; 3-4 tapping, .96; 4-4 Duo-Art, .90; and 3-4 Duo-Art, .71. These values are probably much higher than the true reliability figures because of the factor of immediate memory. It is possible that certain of the subjects recalled in part their former lever settings although an attempt was made to circumvent this possibility by changing the setting before each trial so that once it would be at the extreme right end of the slot, once at the extreme left, etc. It was found that the tempo dial readings of the Duo-Art had no consistent metronome equivalents. That is, identical settings would not render identical tempi for different pieces. It was also discovered that the tempi of the several portions of each piece varied a bit. The variations were slight

[2]For a discussion of this instrument as a laboratory instrument, see Farnsworth (1). For a good sample of its use in actual research, see Heinlein (3).

[3]The subjects were allowed to tap or shuffle their feet if they so desired.

for the waltzes but considerable for "Time On My Hands." In all four of the pieces the rate was less at the end. To make the Duo-Art scores accurate, therefore, the dial scores were recorded on the polygraph in terms of taps per minute as were the telegraph-key responses.

TABLE 1

Type	Mean	Sigma	N	To be compared	D/σ_D	r
4-4 Tap	120.88	27.52	20	4-4 Tap *vs.* 3-4 Tap	.58	
4-4 D.A.	142.72	9.12	20	4-4 D.A. *vs.* 3-4 D.A.	3.23	
3-4 Tap	126.72	30.72	15	4-4 Tap *vs.* 4-4 D.A.	3.91	.43
3-4 D.A.	115.32	31.80	15	3-4 Tap *vs.* 3-4 D.A.	1.36	.46

Table 1 should be interpreted as follows: The average number of taps per minute for 4-4 tempo (telegraph key) was found to be 120.88 with a sigma of the distribution of 27.52 (N, 20). The "proper" 4-4 tempo for the Duo-Art was found to be significantly faster than the "proper" 3-4 tempo (D/σ_D, 3.23), etc.

TABLE 2

Type	Mean	Sigma	N	To be compared	D/σ_D	r
4-4 Tap	136.00	40.64	20	4-4 Tap *vs.* 3-4 Tap	.31	
4-4 D.A.	136.00	19.60	20	4-4 D.A. *vs.* 3-4 D.A.	3.56	
3-4 Tap	132.60	28.62	20	4-4 Tap *vs.* 4-4 D.A.	0.00	.39
3-4 D.A.	113.52	20.28	20	3-4 Tap *vs.* 3-4 D.A.	3.58	.57
4-4 Tap R.	147.28	53.84	20	4-4 Tap *vs.* 4-4 Tap R.	1.05	.52
4-4 D.A.R.	143.84	20.84	20	4-4 D.A. *vs.* 4-4 D.A.R.	2.17	.68
3-4 Tap R.	134.82	43.80	20	3-4 Tap *vs.* 3-4 Tap R.	.36	.79
3-4 D.A.R.	117.54	11.34	20	3-4 D.A. *vs.* 3-4 D.A.R.	1.47	.85

The *R* stands for retest. The four retest correlations probably represent fairly well the reliability values.

TABLE 3

Type		D/σ_D
4-4 Tap	Table 1 *vs.* Table 2	1.38
4-4 D.A.	" " " " "	1.39
3-4 Tap	" " " " "	.58
3-4 D.A.	" " " " "	.19

In a letter to the senior author, Frank Milne of the Recording Department of the Aeolian American Corporation has stated: "We consider 116 to the quarter note to be the most proper for waltz time." It is interesting to note that the subjects of this experiment rendered scores rather close to this value (115.32, 113.52, and 117.54). Mr. Milne would not hazard a guess as to the proper fox-trot tempo, however, for it is known

to have little constancy. A complicating factor exists in the fact that although 64 per cent of a group of 75 students (other than the subjects of these experiments) felt that fox-trot time was 4-4 tempo, it is customarily written *alla breve*. This latter has been variously interpreted as covering almost all of the ordinary tempi with the exception of triple time. One cannot state in absolute time units exactly what it means. The data presented here on fox-trot tempo, therefore, are undoubtedly equivocal in some degree.

The following general conclusions receive at least tentative support from the experiments reported above.

1. The mean of the judgments of unselected university students on "proper" waltz tempo is about the figure proposed by the Aeolian American Corporation. The judgments were measured by the manipulation of a Duo-Art tempo lever while listening to the playing of two music rolls.

2. Similar procedures with fox-trot rolls yielded fairly stable mean values. However, since there are apparently a number of fox-trot tempi and only two Duo-Art rolls were employed in these experiments, the data are undoubtedly equivocal.

3. Although there is no assurance that true reliabilities were obtained, the evidence points to figures sufficiently high for group work.

4. When asked to tap waltz and 4-4 tempi on a telegraph key, scores somewhat similar to those obtained by the Duo-Art procedure were procured.

5. According to the averages obtained, the "proper" pace for 4-4 tempo by the Duo-Art is significantly faster than that for 3-4 tempo.

6. Moderate positive correlations were found to exist between the tapping behavior and the setting of the Duo-Art tempo lever, i.e., between the more motor and the more sensory aspects of tempo.

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RETROACTION AND THE METHOD OF RECOGNITION

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A question of some importance for the theory of memory is whether or not retroaction can be demonstrated to exist when retention is tested by the method of recognition. Heine (2) obtained no retroaction using this method. If her results are accepted as final, then it must be believed that recognition memory is fundamentally different from recall (or relearning) memory in that it is not susceptible to retroactive inhibition. But Dahl (1) has found that the fact of better retention after a period of sleep than after one of waking holds also when the retention is tested by recognition. With reference to the "sleep-effect," then, the two methods of testing memory give the same results. Dahl reasons that since the sleep-effect holds for both recall and recognition, and since (accepting Heine) the retroaction effect holds for recall but not recognition, the two effects must be distinguished. Hence the fact of superior retention after a period of sleep cannot be explained in terms of retroaction.[1]

This conclusion of Dahl's makes the interpretation of the sleep experiments (6, pp. 415-439) a difficult problem. It also destroys their value as evidence for the theory that forgetting is in large part the result of interpolated activity.[2] Since the whole argument rests upon the correctness of Heine's finding that retroaction does not occur with the method of recognition, there would seem to be value in questioning Heine and reopening the problem to experimental investigation. Especially is this true in view of the evidence, not wholly conclusive, from the experiments of Strong (5) and of Lund (3). They both employed a recognition technique and both found evidence of retroactive inhibition in their results. The present experiment[3] is designed to test for retroaction under conditions which seemed to the writer more favorable for its appearance than those employed by Heine. The recognition method as developed by Lund was adopted; this modification of the orthodox method, by making the items harder to discriminate from their context, increases the chances of erroneous recognitions and may be said to be a more exacting test of recognition memory than is the usual method. Lund's material, shown in Table 1, was borrowed for the present experiment. The nonsense words of Series A, printed on cards, were presented serially to the individual subjects for 5 seconds each, with instructions to look at each word carefully and to

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[1]For an account of Dahl's conclusions see Van Ormer (6, pp. 425-427).

[2]See McGeoch (4).

[3]The writer is indebted to Eleanor J. Gibson for suggesting the need of this experiment and to Lucile Hudson and Ruth Hausmann for their aid in carrying it out.

TABLE 1
MATERIAL USED IN THE EXPERIMENT

Original words for memorization Series A	Confusion words with which original words were mixed for recognition Series B	Series C
GATOR (13)	GATO*N* (9)	GAT*EN* (6)
MAREN (16)	MARE*R* (12)	MAR*UR* (8)
DULEN (22)	*B*ULEN (19)	*SA*LEN (17)
KUNEL (29)	*S*UNEL (27)	*GA*NEL (26)
AUMON (30)	AU*L*ON (28)	AN*L*ON (23)
FABAS (25)	FA*L*AS (20)	FE*L*AS (18)
SETIR (24)	S*A*TIR (15)	*BA*TIR (10)
BIDAR (21)	B*E*DAR (11)	*KE*DAR (4)
PAMOR (14)	PAM*IR* (7)	PAM*IN* (2)
CELOM (5)	CEL*UM* (3)	CEL*UD* (1)

try to remember as many as possible. The series was presented only once. Subjects were told that their memory for the words would be tested but they were not told by what method. Next came an interpolated period of 5 minutes. During this period the *experimental group* learned or partly learned, by means of successive presentations, another series of 10 nonsense words made up like those of Series A, but bearing no specific resemblance to them, e.g., LODUS, HINAP, JUPOF. Each card was exposed for 5 seconds, as before, and at the end of the series subjects were required to write as many words as could be recalled. In 5 minutes there was time for about 4 presentations of the series and the average number of words learned by the time of the last recall was 4.8. The *control group* during the interpolated period was instructed to rest and look through a book of colored prints containing no reading matter except titles.

At the end of the interpolated period all subjects were presented with a list of 30 nonsense words, printed on cards, one every 5 seconds, with instructions to report those which they recognized as being from the original list. Ten of these 30 were the words of the original list (Series A); 10 were words with one letter changed from the corresponding word in the original list (Series B); and 10 were words with two letters changed (Series C). The order of presentation is indicated in Table 1 by the numbers in parentheses. It should be noted that in this order of presentation the correct word was always preceded by its two similar but incorrect words with the least similar first, or stated in another way, that the items of Series C preceded those of Series B, and these in turn those of Series A. Consequently, it was possible for a subject to recognize incorrectly one or both of the words of Series C or B and later to recognize correctly the corresponding word of Series A. In such cases the subject either realized his earlier mistake, or thought that the series contained duplicates. Incorrect recognitions, or errors, were thus of two types: errors subsequently

corrected, and errors not subsequently corrected. The latter type was less frequent.

A recognition *score* was calculated for each subject. One point was given for each correct recognition and a half-point was deducted for each error of the first type, i.e., for false recognitions in those cases where the correct word was later designated. The highest possible score was thus 10 and the lowest zero.

Twenty subjects were tested in the experimental group and 20 more in the control group. The results may be seen in Table 2. There is a significant difference between the groups in the amount of retention shown, both according to the recognition scores and according to the number of errors of both types made. By both of these criteria the experimental group was inferior. On the average, the experimental group scored 31% less in recognition and made 53% more errors than the control group.

TABLE 2
RESULTS

	Experimental group (learning interpolated)	Control group (pictures interpolated)
Av. recognition score	3.3	4.8
Av. errors (corrected)	3.6	2.6
Av. errors (uncorrected)	1.6	0.8
Av. errors (total)	5.2	3.4

The results of this experiment, in contradiction to those of Heine, indicate that a significant amount of retroactive inhibition may appear when retention is tested by the method of recognition. Although more experimental evidence is needed before the issue can be decided, it is at least clear that Heine's results cannot be accepted as *proving* that retention tested by recognition will under no conditions show a decrement as a result of interpolation.

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PERIPHERAL FORM DISCRIMINATION UNDER DARK-ADAPTATION—ERRATUM

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In the October, 1933, issue of this journal (Vol. IX, No. 2) Figure 1 and Figure 2 appear in transposed order. The diagrammatic representation appearing above the legend "Figure 2" on page 413 should accompany the legend "Figure 1" on page 409, while the graphic representation of results appearing above the legend "Figure 1" on page 409 should accompany the legend "Figure 2" on page 413.

APPARATUS

THE REFLEXOHMETER (POCKET TYPE)[1]

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The need for an apparatus which would indicate galvanic reactions to actual life situations outside the laboratory led to the construction of this instrument. It offers a pocket-size Behavior Research Resistance Box (1) plus a highly stable and moderately sensitive galvanometer (about 0.6-mm. deflection per 1,000-ohms change) for observing resistance levels and galvanic reactions. This apparatus may be carried in the coat pocket or it may be carried open in the hand, or resting on the lap, wires being run up the coat sleeve from electrodes on one hand. The subject may observe his own reactions, or an observer may accompany the subject, recording resistance levels, reactions, and the nature of the stimulating situations.

The view of the instrument in Figure 1 is largely self-explanatory. The Behavior Research Resistance Box circuit with a recording range of 100,000 ohms is here used with a small microammeter (*A*), empirically calibrated to read in 1,000-ohm steps. An adjustable standard series resistance in 20,000-ohm steps (*B*) permits maintaining sufficient resistance in the subject's arm of the bridge to keep the galvanometer within the dial range. Three vest-pocket flash-light batteries give a uniform current of .0409 milliamperes through the subject's arm of the bridge at balance. Switches (*C*) and (*D*), respectively, permit turning the battery on and off and switching the subject in and out. A third switch, not shown, affords protection (shunt) for the galvanometer. Telephone jacks (*E*) make connections to the electrodes. As in other forms of the Behavior Research Resistance Box, the subject's resistance at any time equals the 100,000-ohms recording range (balance) minus the resistance in series with the subject, plus or minus (as the case may be) the deflection in ohms indicated by the galvanometer pointer on the scale.

The wide applicability of this stable pocket-size galvanometer equipment is shown by a preliminary study of galvanic reactions while automobile driving. The powerful effect of any situation requiring the mobilization of the driver's energies, such as the sudden slowing of a car in front, the attempt to pass another car on the road, the sudden appearance of a pedestrian in the road, or the sound of a policeman's whistle, is clearly demonstrated. Observations on the reactions of passengers to different situations are also of interest. The study of the resistance levels and the galvanic

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[1]Studies from the Behavior Research Fund and the Institute for Juvenile Research, Chicago, Series B, No. 193.

reactions (larger than 1,000 ohms) during the actual activities and perturbations of daily life rather than within the limitations of artificial laboratory situation is made possible.

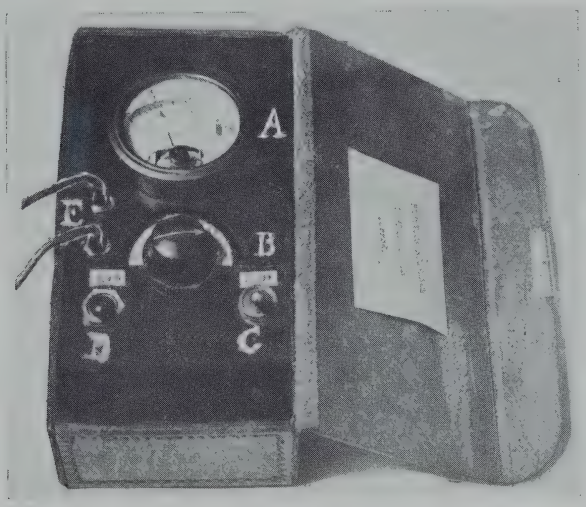

FIGURE 1

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BOOKS

EDWIN G. BORING. *The Physical Dimensions of Consciousness.* New York: Century, 1933. Pp. xii+251.

Anyone who listened to Professor Boring's New Orleans pronouncement that "dualism is dead; it ought to be buried" became immediately aware that something of importance was happening within the Titchenerian tradition. A careful study of the printed address[1] convinced him that the event was not only important but exciting, and perhaps raised some doubts as to whether the change was more than accidentally within that tradition. Now comes a book, of which the paper was but a foretaste, maintaining in a highly consistent manner the doctrine that consciousness is a physiological event; that psychological phenomena are describable in terms of physical dimensions. Others have written polemics in defense of an identity theory of consciousness; it has remained for Boring to make the assessment of its fruitfulness for experimental psychology. Since this is done concretely, most of the more important psychological problems being taken up in a systematic fashion, the reader should not be surprised to discover that the book may be extremely useful as a psychophysiological text.

Boring's chiefest concern is to avoid the "epistemological vertigo" to which introspective psychology has succumbed by conceiving itself to be propaedeutic to physics, since physical entities are inferentially derived from "direct experience," and at the same time to be systematically posterior to physics through its identification of the "experiencing individual" with one part of the physical world, the nervous system. A scientific psychology can resolve this dilemma only by renouncing the dualism that has encumbered it; it must embrace a consistent monism as do all other branches of science. This goal is not to be achieved by naïvely adopting a phenomenology, for "even the view that mind is the only reality (Berkeley) becomes dualistic when it leads to the problem of how a knowledge of an external world can come about." Neither is behaviorism the answer, for "the thoroughgoing dualist is the naïve behaviorist who ignores consciousness, thus asserting that there is something to be ignored in a world so different from his own that he can safely disregard it." The scientific sophisticate will see that a monism is to be achieved only through a recognition of the fact that psychological reals, like physical entities, are products of inductive inference, that realities are hypothetical constructs. These, in turn, depend upon systematization of experimental data. Just as atoms and electrons are or have been real entities because empirical facts have made them so,

[1]Boring, E. G., The physiology of consciousness. *Science,* 1932, **75**, 32-39.

the realities of psychology—emotions, ideas, memories, and the like—are constructs inferred from experience. "One does not attempt to discover conscious elements, attributes, or dimensions; one makes them up and uses them as phenomenological exigencies require." Already the psychology of sensation has pointed the way; other "realities" may be expected to change as experimental data force revisions.

Since "consciousness turns out to be, if one speaks accurately and carefully about it, sensory," the problems raised by any systematic approach to the description of conscious events must of necessity be in terms of the psychophysiology of the special senses. Any sensory field is organized with respect to four dimensions: quality, intensity, extensity, and protensity. "The dimensions of consciousness are the immediate successors to the old attributes of sensation. . . . Titchener's fifth dimension, *attensity,* has, in the view of the present author, become unnecessary. At any rate we can ignore it for the time being." Boring does not make it clear at once as to why attensity is to be dropped. Is it because, as some have maintained, attention is coextensive with consciousness itself, or does the experimental psychology of attention fail to yield any substantial conceptional reals? The reason for Boring's dismissal of attensity is not discovered until one comes to a note near the end of the book. Discussing the development of the notion in the hands of Titchener and Dallenbach he says: "The present author believes that it is in the public interest to take the term *attention* for cognitive clearness. It is not even clear that the legal title does not lie there. . . . Whether attensity is a necessary dimension of consciousness, future study will show. At present it seems to have little systematic significance. Nothing serious happens to a system of psychology if the concept of attensity as a structural, sensory, descriptive category is left out." The suspicion is strong that the experimental psychology of attention, with its faltering techniques, has brought about its own indictment.

Lest it be concluded that the author has contented himself with a restatement of problems of general theory, and in particular with the mind-body problem, it should be hastily pointed out that the major portion of the book is devoted to the exposition of experimentally determined facts and the raising of problems that are susceptible of experimental solution. The story of the dimensions is the story of experimental psychology. Nor is it exclusively sensory psychology. In the chapter on "extensity" there are introduced the problem of brain localization and the very relevant data of Lashley. From this point to the end there is to be found a continuous thread of reference to "mass action" and "equipotentiality." The discussions of attention and learning are colored by these concepts; much of the view on intelligence is a direct outgrowth of a consideration of principles developed by Lashley and his colleagues. Boring, in the preface, apologizes for having used these theories in a "wanton way." If asserting in a courageous and

open fashion those things that perforce follow upon the acceptance of a theory is wantonness, we have need for more of it in psychology. The chapter on the "Organization of Consciousness," which has most to say of these things, is brilliant and should be read by all who may have privately concluded that these psychological topics are exclusively in the hands of racketeers.

As in Professor Boring's excellent "History," the more intimate information is to be found in the copious note sections following each chapter. The style is here almost conversational, in fact, revealingly autobiographical. One who passes by the notes as unnecessary baggage will miss the most provocative discussions to be found between the covers of the volume.

Boring will undoubtedly be credited by many readers with having exposed a "system" of psychology and he will undoubtedly stoutly deny that his interest is in system-building. It was so when once before he attempted to bring to a focal point the trends in theory, that time in a special sensory field. Despite his protests, references are made to Boring's "auditory theory" rather than to his auditory theorizing. One may confidently expect to find mention of Boring's "system of psychology" when the whole effort is one of "systematizing," raising problems rather than settling them.

The enthusiasm with which the book is even now being received, not only by psychologists but by logicians and metaphysicians, is itself sufficient commentary upon its worth. The very fact that its consideration leads to fruitful discussion renders without point any further evaluation by the reviewer.

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J. G. BEEBE-CENTER. *The Psychology of Pleasantness and Unpleasantness.* New York: Van Nostrand, 1932. Pp. viii+427.

It is a matter of some difficulty to arrive at an intelligent evaluation of any book that attempts to coordinate and systematize the results of diverse efforts to uncover facts, particularly when the subject under consideration is one that does not yield readily to experimentation. The psychology of affection is such a field. Workers in it have groped about in the semi-darkness for decades and the light is not yet.

Emphasis in this volume is entirely upon experimental data rather than upon speculation and field observation. After clearing the ground in the first chapter by defining "hedonic tone" as "the general algebraic variable, whose positive values correspond to pleasantness and whose negative values correspond to unpleasantness," the author describes in some detail all the known available methods for its investigation. Then in successive chap-

ters there are considered relations of hedonic tone to mental elements, primary external stimuli, secondary external stimuli, motivating factors, maturation and learning, muscular and glandular responses, memory, and nervous processes. The final chapter treats of the various theories of hedonic tone. When it is considered that in connection with each of these topics large numbers of researches are critically examined, something of the magnitude of the task set by the author is appreciated.

The general problem of the relation of pleasantness and unpleasantness to the elementary analysis of consciousness is a matter of deep concern to the author. He has gone over the whole ground so thoroughly worked by Titchener in his famous *Lectures on the Psychology of Feeling and Attention* but from the vantage point of the present. There is, however, the difference that the systematic outcome of his appraisal was for Titchener a matter of great moment. The present author is able to conclude that we are not yet in a position to construct mental elements, hence the problem of the relation of hedonic tone to the elements does not exist. In fact, it is regarded by him to have been an unfortunate occurrence that psychologists ever became interested in such a spurious relation. Why one should deplore history is not quite clear; modern experimental problems, after all, are pretty much historically determined. The entire book, paradoxically enough, bears witness to that.

The twenty-year quarrel over the validity of Wundt's tri-dimensional theory is carefully traced. Ostensibly the end of the argument is a stalemate rather than a victory for either camp. Beebe-Center, however, prefers another interpretation: ". . . . special affective elements are equivocal with respect to the number of their qualities—that they are three-dimensional in Leipzig but uni-dimensional at Cornell." This instance is but one of many among the problems of affective psychology. Strikingly different conclusions are reached by different workers, yet one is at a loss to choose between them. This situation comes about not by the adoption of faulty techniques which fail to yield clear answers to experimental problems but through setting out from different positions in general psychological theory. "The conceptual entities constructed by psychologists depend upon general systematic backgrounds. As this background varies from one psychologist to another, the conceptual entities will vary likewise. . . . experimenters with different backgrounds must interpret their data in ways which yield conflicting conclusions. The views that hedonic tone is an attribute of sensations, of complexes of sensations, of qualitatively unidimensional affective elements, of qualitatively pluridimensional affective elements, are not inconsistent descriptions of experience, but alternative hypotheses constructed from experience. Again the views that affections may and may not give rise to mixed feelings, are and are not clear, localized, or extended, do not constitute pairs of contradictory characterizations of a single entity,

but independent characterizations of different entities." Thus Mr. Beebe-Center, stressing in an equally forceful manner the epistemological position adopted by Professor Boring in his recent book, reveals his own part in the "perpetual colloquium which exists in the Harvard Psychological Laboratory" for which Boring has so graciously given thanks.

The same laboratory, though evincing much interest in the view of Nafe and being responsible for the only clear external support of it by experiment, is very slow to accept Nafe's theory. The author makes out a good case for it, seems on the point of concluding in its favor, then shelves it. Perhaps he shares the skepticism of his colleague, who has recently shown considerable annoyance at Nafe's "radical" theory of feeling, the necessary logical outgrowth of the earlier work on affective analysis. Perhaps, too, he shares his colleague's complaint of not being able to understand it from the written accounts. The classification of the theory as one tied up with elementarism (Chapter XI) leads to this suspicion. All this seems the more remarkable when it is found that the "heuristic hypothesis" put forward by Beebe-Center is found to differ from Nafe's theory only by process of hair-splitting. Considerably more interest would have accrued to certain sections, notably those dealing with relations of hedonic tone to external objects, had the author been less concerned with completeness and more with weeding out the shoddy and inconsequential work. One assumes heavy responsibility when he essays evaluation of the research of others, but once attempted, and a work of this kind must do it, there is much to be said for arriving at a proper perspective. The author could have been more critical and with good reason. For the most part, the section dealing with the relations between "qualitative" characteristics of stimuli and affective judgments is the recital of ill-conceived and wretchedly controlled experiments. These things are, of course, no fault of the author. However, he obviously considers them important enough to reproduce all the dull details. Not until he arrives at Chapter VIII, where the results of the use of the method of impression are reviewed, does he tire of them himself; there he complains that lack of space necessitates confining attention to only the more outstanding investigations. This resolve should have come earlier.

Perhaps through the operation of the somewhat asthenic "law of hedonic contrast" there are some parts of the book that are entirely engrossing. Thus to determine whether there is an invariable relation between primary stimulus and hedonic tone Beebe-Center makes the novel application of Spearman's tetrad-difference technique to the problem. On the basis of somewhat inadequate empirical data he is able to show that the principle has value and that there does exist the expected relation. Of all the technical refinements suggested in the book this one is the most interesting and promising.

This is unquestionably a book that will have to be reckoned with and, to a certain extent, relied upon by researchers in the field of affection, whatever its interest to the general reader. After all, perhaps one should expect a systematic review of this kind to give way to explication of the obvious and a general redundancy; perhaps one should expect it to score more heavily on the side of completeness than succinctness.

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